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## CONGENITAL LUXATIONS OF THE FEMUR,

### A REPORT ADDRESSED TO THE ROYAL ACADEMY OF MEDICINE.

*By* M. GERDY.

OUR knowledge respecting congenital luxations of the femur is far from being perfect, and, in all probability, many varieties of this condition remain still to be ascertained. The facts accumulated since attention has been directed to this subject are, as yet, too limited in number to elucidate it in all its bearings; but they, at least, show that there are numerous varieties of congenital lesions of the hip-joint produced by malformations, lesions inducing permanent or temporary displacement of the articulating surfaces. I shall endeavour, as I proceed, to point out such obscure points as require investigation; to examine them fully would occupy more time than we can spare for the purpose.

Under the name of congenital or original dislocation, all cases have been included, in which permanent luxation or displacement of the head of the femur has been affected through a vicious conformation or development of the parts concerned in the articulation. They are thus termed because they frequently exist at birth; but too much importance must not be attached to this name, neither must all luxations, anterior to birth, be thus characterised, nor all produced subsequently to birth be, on the other hand, excluded from the category of those caused by a vice of development or conformation. Experience and rational induction prove that certain dislocations, some of which have occurred prior, and others subsequently to birth, resemble each other strongly, and depend on the same cause.

Thus Dupuytren relates, in his *Leçons Orales*,* the case of an old woman of Nantes, several members of whose family, both among her progenitors and her descendants, were affected with congenital dislocations; and who herself, though under the influence of an hereditary disposition, received and transmitted, lived to her thirtieth year before a spontaneous luxation manifested itself. Can there be any doubt that, in the case of this woman, the displacement was prepared and produced by the same cause that determined its occurrence in the other members of her family? I am well aware that when a dislocation, which has been prepared in advance by a vice of development, occurs at the moment of birth, or after that event, it will, in appearance at least, be confounded with traumatic luxations; but the occurrence of embarrassing cases cannot be prevented.

The subject of congenital luxations has been handled by a few authors only. They are, however, mentioned in the books attributed to Hippocrates.

It seems extraordinary that, after having been thus distinctly indicated, as well as their principal signs, they should have been subsequently altogether forgotten. This has proceeded from the authors of the writings referred to not having broadly drawn the line of distinction between traumatic and congenital dislocations, and to their having described together the symptoms of both species of lesion. Avicenna and Paré also mention them, but in a much vaguer manner. The younger Verduc, on the contrary, has spoken of them in so precise and positive a manner, that I shall read a passage from his writings, in order that, by and by, we may be better enabled to judge of the claims of our contemporaries to discoveries in this branch of the science. After having described the dislocations of the thigh, and the methods of reducing them, Verduc expresses himself thus:—"Before practising extension ascertain carefully the nature of the dislocation; for, if your patient have been lame *from birth*, extension will serve no other purpose than to expose your ignorance. I cannot select an example better illustrative of this than the 61st case related by Kerkring. One of his nieces, who was lame, was brought to a bone-setter. This person seized the leg with his hand, in order to make it of the same length as the other, but

* T. iii., p. 217.

the moment he let it go it assumed its original length, and the child limped as before. She was brought to several other operators, who all displayed their ignorance..... Kerkring himself allows that he could not discover the cause of the peculiarity until after the child's death. He then found that the cavity of the ischium was very wide and deep, and that the head of the femur was, on the contrary, extremely small, so that it perpetually slipped out of the former. The ligaments were relaxed, and, being elongated by the weight of the thigh, the head of the femur, instead of remaining in the articular cavity, presented itself indifferently in every part of the surrounding surface. But extension rendered the limb of the same length as its fellow, because the head of the bone then entered the articular cavity; an apparent cure was thus effected, but matters returned to their previous state the moment the limb was let go. This case leads to the conclusion that the majority of individuals lame from birth, either on one or both sides, have a hip-joint of the structure observed in Kerkring's niece."* Paletta, who has devoted sixty-three quarto pages of his *Exercitationes Pathologicæ* to congenital claudication, does not refer to the head of dislocation the lesions of the coxo-femoral articulation, which he had occasion to observe. He exerts himself to show that they are distinguishable therefrom by very different characters; for example, by their depending on a defect in formation, while luxations are the result of an external violence. This author relates with considerable fulness the seven cases which he had witnessed and studied. The first of these refers to an infant for whom he was consulted; he recommended the use of astringent topical applications to the joint, and of a bandage calculated to restrain the ascent of the femur. The subject of the second case was an infant, aged seventeen months; of the third, a young man, aged twenty years, whose articulation he had an opportunity of dissecting; the fourth case referred to a little girl, four years old, whom he also examined after death; the fifth, sixth, and seventh, to one woman aged upwards of fifty, to another still older, and, lastly, to a man also examined after death. In the cases in which this distinguished professor dissected the joint after death, there was a real displacement of the head of the femur; in the fifth, sixth, and seventh, it would appear that the shortening of the limb depended altogether on the state of the neck of the femur, which was too short and too horizontal. Andry also makes mention of congenital dislocations in his *Traité d' Orthopédie*, but he does not describe them. Monteggia bestows a few lines on them in his *Institutes of Surgery*.

* De la Luxation de la Cuisse, t. i., p. 570.

Among ourselves, Dupuytren has contributed more than any one else to direct the attention of practitioners to congenital dislocations, and to lay the foundation for their history; but he is far from having completed it, and he is not the first who was acquainted with these affections. In vain did he assert, in his memoir published in 1826, in Breschet's *Répertoire d'Anatomie*, "there is a species of displacement of the upper extremity of the femur, *of which I have not been able to discover the least notice in authors, in spite of all my inquiries.*" The facts just referred to prove that these inquiries were exceedingly limited; so much so, indeed, that it is fair to doubt their ever having been made. Dupuytren, I have said, has by no means completed the history of these lesions, for the more profoundly they are studied, the greater is found to be the number of varieties with which he was utterly unacquainted. The congenital lesions of the hip-joint are so numerous and so varied, that it would perhaps be necessary, in order to draw up a complete history of them, to unite into a group cases of luxation with the congenital malformations of the articulation, which entail obstruction in its movements, and among others produce claudication. The common title of congenital malformations of the ilio-femoral joint would answer very well for these lesions, and would thus unite into a natural group a number of affections, of which dislocations would form a subdivision. By imitating Dupuytren, we separate from congenital dislocations analogous cases in which no luxation exists at all, or exists only momentarily. Thus individuals are met with who can dislocate and reduce their thigh at will. Portal speaks, in his *Anatomie Medicale*, of an Abbè of St. Bonnet, who possessed this power. There is a medical practitioner at present residing at Troyes, who, according to the statement of MM. Humbert and Jacquier, can perform a similar feat.

Dupuytren has not only omitted, in his description, a number of facts mentioned before he wrote or subsequently observed, but he has incorrectly generalised the occurrence of particular symptoms, which experience proves may be either simply absent, or replaced by others. Thus, according to his essay, the foot is turned inwards; whereas, according to nature, it is sometimes turned inwards and sometimes outwards. Rest, refrigerants, and the use of a pelvic bandage, are the only methods of treatment which, like Paletta, he recommends for this affection; he regarded it as incurable.

The *Orthomorphie* of Delpech, which appeared in 1828, added nothing new to what was already known; if we except the sentence in which the author remarks that there must have been "a deficiency in length in all or the greater number of the muscles of both the lower extremities," in such of

Dupuytren's patients as walked on the point of their toes. MM. Breschet and Cruveilheir have also given their attention to congenital dislocations; and, in 1833, M. Pravaz drew up an abridged history of them for the *Dictionnaire de Medicine*, article *Bassin*.

So far these complaints had been examined in a purely scientific point of view; nothing had been done for the art, when M. Humbert in 1828, conceived the idea of applying his orthopœdic instruments to the reduction of a spontaneous dislocation. Although this attempt was not very rational, it opened a new path to the art of surgery; for it led M. Humbert to make trial of his machines for the reduction of congenital dislocations. In 1833 he presented a memoir to the Institute, relating the results he had obtained. A commission of that learned body examined a certain number of patients with congenital dislocations, consigned them to the professional care of M. Humbert, and subsequently, from the representations made a year afterwards of the improved state of those patients, decreed him the Monthyon prize. Nevertheless, the medical world neither considered the pretensions of M. Humbert, to the merit of curing the disease, well-founded, nor themselves endeavoured to cure spontaneous or symptomatic dislocations by mechanical extension. His work, published in conjunction with M. D'Evry, and entitled *Essay or Observations on the Method of Reducing Spontaneous Luxations of the Hip-joint*, is, notwithstanding, a good book. M. Pravaz, who was at first incredulous, thought fit subsequently to attempt the cure of these affections with machines; but he soon published, as the result of his trials, his conviction that M. Humbert had not reduced a single luxation of the femur, but simply transformed iliac into sciatic dislocations. In this view he was joined by M. Joffre and many others. M. Bouvier has gone further, affirming that M. Humbert had not even changed the character of the luxations, but left them precisely as he had found them. In April, 1834, M. De Mellery defended a thesis on congenital dislocations before the faculty of Paris; this was simply a reproduction of the ideas of Dupuytren. The same year, the son of the celebrated Sandifort presented another at Leyden, on the same subject. This is a work of much greater talent: the author takes a wider view of the subject than Dupuytren had done; in all probability recognising the affinity subsisting between all the vicious conformations of the acetabulum, of the head and neck of the femur, and of the ligaments and congenital dislocations; he treated of all these points in his thesis. It is true he adds thereto a long dissertation on the fracture of the neck of the femur, which has no connection with the other subject. Be this as it may, the thesis is a very interesting production, and contains in a small compass a variety of facts. M. Sédillot commenced the publication of a memoir on femoral dislocations, *studied on the dead body*, in December, 1838, and continued it in January, 1839. If we add to our list an excellent report presented to the Medical Society of Lyons last May, we shall have a complete list of the principal works hitherto published respecting the affection of which I now proceed to examine the anatomy, pathology, and therapeutics.

*Anatomical Characters.*—Congenital luxations of the femur are either single or double. They do not consist simply in the permanent displacement of the head of the bone from its articular cavity, but in a number of local and surrounding changes, some of which affect parts at a considerable distance. Hence we have essential or local lesions, and distant accessory lesions. The *essential* lesions affect the superior extremity of the femur, the old and new articulating parts of the ileum, the cartilages, the ligaments, and the muscles surrounding these structures. The *accessory* lesions also affect the bones and investing muscles; namely, the body of the femur, the ileum, and the muscles of the pelvis; and distant parts, viz., the vertebral column, and the entire lower extremity.

The *superior extremity of the femur* frequently presents lesions of variable character, like those to which I have just referred. The head of the bone is often smaller than natural, or flattened from before to behind, according to the direction in which the limb is turned. The diminution of size, thus occurring in the head, is frequently met with in the neck of the bone also, which is shorter and less thick than in the healthy state. In other instances these two parts are replaced by a simple styloid eminence, or a flattened elongated protuberance. Or, again, the primitive lesion preventing the normal contact of the bones, may exist at the point of union of the neck and body of the femur. Thus, while the neck of the femur, in the healthy state, is inserted into the internal surface of the bone, and is directed from without inwards, it has been observed in abnormal cases rising directly from the anterior surface, and passing from behind forwards; in other instances issuing from the antero-internal surface, and pursuing an oblique course from behind forwards, and from without inwards. These facts are related by Paletta and Sandifort. It will be readily understood that, as in such cases, the axis of the acetabulum, and of the head of the femur, follow perfectly different directions, the normal relation of the bones cannot be maintained. The body of the femur is displaced in regard of its circumference, as if it had undergone a movement of rotation on its longitudinal axis, while its upper articular extremity is turned in the contrary direc-

tion. Under these circumstances, there frequently appears to be complication of club-foot, although such lesion is not in reality present. In still more serious cases, the head and neck of the femur are wanting, the upper end of the bone terminates with the trochanter major. Three such cases may be seen in the Museum of Morbid Anatomy of the Faculty: one of these, presented by M. Breschet, belonged to a man aged 46; another was procured from the body of an adult, and existed on the left side; the third, also belonging to an adult, existed on both sides. I might to these cases add a fourth, in which the upper extremity of the femur is ossified, and incorporated with the antero-inferior spine of the ileum. Need I remark, that for such cases there is no possible remedy?

The articulating parts of the ileum, which become united to the femur, are the cotyloid cavity (acetabulum), and a new or supplementary cavity. The acetabulum is usually decreased in width, and otherwise deformed to a variable amount. It may be almost completely effaced, and reduced to a slight depression, either wide or narrow. The narrowing and other alterations in its form are especially remarkable, when the head of the femur lies on its border, and forces it downwards and inwards; or when this border has become thus spontaneously inverted, a condition of which examples are to be seen in the Museum. The acetabulum may, on the contrary, consist of a simple bony prominence, surmounting a superficial depression, or one disgroved in the form of a narrow sulcus, only partially surrounding the prominent rim. Two pelves of this description may be seen in Dupuytren's Museum. It is more usual to find the acetabulum somewhat spacious, either circular or elliptic in shape, triangular or irregular, diminished in depth to an amount proportionate to that of its decrease in width, communicating internally with the foramen ovale by a wide notch, and filled wholly, or in great measure, by a mass of cellulo-synovial tissue, which is in some cases diseased and fungous. M. Simonin has even observed a spontaneous luxation, co-existing in a young girl of eleven years old, with an acetabulum sufficiently large to lodge the head of the femur. In cases of this description, the dislocation appears to be caused by abnormal and excessive length of the capsule of the joint; and the possibility of reducing such luxations may be readily conceived, provided the elongated capsule has not undergone contraction between the head of the femur and the acetabulum. This cavity has occasionally been observed of greater size than necessary for the reception of the head of the bone. Kerkring's case, already cited, furnishes a memorable example of this condition. Besides, sometimes the head of the femur lies outside the acetabulum, between the iliac spines and the sciatic notch; in other cases, it lies on the upper border of the cavity, or slips in and out of it with facility.

The new or supplementary cavity is often placed above the acetabulum, between the summit of the sciatic notch and the anterior spines of the ileum; the exact height and degree or proximity to the former or latter of these points differing materially in various cases, as the specimens in the Museum of the Faculty show. This cavity usually forms only a slight depression, surrounded by an irregular osseous prominence, secreted round its upper border, and occasionally to a greater extent. This prominence is, however, regular and even smooth on the side corresponding to the head of the femur, which it embraces, and on which it lies.

In another set of cases, there is neither a supplementary cavity, nor a bony prominence, on the surface of the ileum, but simply a slight depression, or with attenuation of the bone, where it corresponds to the head of the femur. Or, again, there may neither exist cavity nor depression, the femur apparently exercising no compression on the bones of the pelvis.

The *cartilages* of the articulating surfaces are frequently wanting; in some cases, however, the cartilage has been found partially preserved in the acetabulum, when an opportunity has been had of examining the parts soon after birth. That of the head of the femur, on the contrary, often remains persistently; but it is almost always uniformly attenuated, or even reduced in some spots to mere shreds. As respects the new cavity or iliac depression, if the movements of the head of the femur are extremely free, the bones slipping in different directions, without remaining steadily in any one, the periosteum of the ileum is found in very nearly the natural condition, or merely slightly thickened. But when the relation of the bones is more steadily fixed, and the situation of the head of the femur constantly the same, the periosteum has been found thickened to a much greater extent, and even assuming to a certain degree, if we may credit the relations of authors, the characters of an articular fibro-cartilage, or those of simple fibrous tissue; its filaments raised from the surface of the bone, sometimes resembling the filamentous pile of velvet. When a more perfect pseudarthrosis is established, the iliac periosteum is wanting entirely or in part, or the surface converted into an articular one. This portion of the bone presents an excavation of more or less regularity and smoothness; the head of the femur is without any cartilaginous investment, and the two osseous surfaces move on each other in immediate contact, both of them covered with a thin layer of compact tissue, marked with superficial depressions.

*Ligaments.*—The round or interarticular ligament is sometimes completely absent, the ordinary points of its insertion being

marked by a little excavation, or a small ligamentous prominence: in other instances it is found in the natural position, but always of undue length (*Paletta, Dupuytren*), almost invariably attenuated, atrophous, and sometimes split into several thin bands. (*Dupuytren, Sédillot.*) There is a specimen of considerable elongation of this ligament, in a wax model, in the Museum of the Faculty. The fibrous capsule retains its natural condition; it is single, complete, without change in its points of insertion, without solution of continuity, and simply elongated in the direction of the dislocation. One-half of its length, which is horizontal, embraces the head and neck of the femur; the other, vertical in its direction, is attached outside the brim of the acetabulum; it is consequently bent at right angles on itself. It is besides frequently caught between the head of the femur and the acetabulum; but this is not always the case, so that it is not impossible in some instances to replace the head of the femur directly in the articular cavity. The capsule is sometimes hypertrophous to such a degree as to reach one or two lines in thickness: it is then extremely resistent, and the neck of the femur pressing against it, sustains half the weight of the body, like the fist supporting a weight suspended to the coat sleeve overhanging the clenched hand. In some cases the capsule is displaced along with the head of the femur, and glides on the iliac fossa; and when this occurs, a synovial bursa facilitating its movement is sometimes found between the capsule and the ileum. When a pseudo-arthrosis is formed, the capsule, ending by contracting intimate adhesions with the surface of the ileum, and subsequently undergoing perforation from the pressure of the head of the femur against the iliac fossa, presents, as it were, two distinct pouches, communicating by a wide and direct opening with each other. In this case, the capsule may be shortened, and much thickened, and thus effectually prevent *immediate* reduction of the head of the femur, though it may not resist efforts at reduction, kept up for a length of time, and of gradually increasing force. Under all circumstances, the capsule, and the bony excavations it surrounds, are moistened with synovia.

The supra-articular *muscles* undergo certain modifications consequent on the luxation. The glutæus minimus is distended, compressed, occasionally destroyed (*Dupuytren*), or torn by the head of the femur, which is found lodged in its substance; sometimes the head of the bone passes completely through it, and is then seated under the glutæus medius. As a consequence of the pressure and distension undergone by the muscles, all or only the most deeply-seated of the fibres of those muscles are found to be discoloured, and transformed into a yellow fibro-cellular tissue, of fatty appearance. This tissue is incorporated with the new capsule in cases of pseudarthrosis, and concurs in forming the latter. It is simply attached to the primitive capsule, when this is only distended; in course of time it coalesces with it, thickens, and strengthens it. The glutæus medius also occasionally loses a part of its properties, and becomes unfit for the exercise of its functions.

So much for the principal or articular lesions. We now come to the *secondary* or *accessory* disorders. The inlet of the *pelvis*, pressed between the femora, which rest directly on the surface of the ileum, a less resisting medium than the acetabulum, undergoes more or less marked deviation from the natural form. In some cases lateral sinking of the inlet, with corresponding increase in its antero-posterior diameter is observed; a wax model of a girl thus affected is to be seen in Dupuytren's Museum. In other cases the antero-posterior diameter is the narrower of the two (*Sédillot*); but no general rule can be established in this respect. The tuberosities of the ischia are often widened, sometimes attenuated, angular, turned outwards (*Sandifort*) and forwards, as if they were inclined to place themselves on the same transverse line as the anterior part of the pubis. This circumstance produces a considerable change in the pelvis; its anterior part descends obliquely forwards, and the transverse diameter of the outlet is greater than usual. This peculiarity appears to be produced by the continued traction undergone by the gemelli, the obturators, the quadratus femoris, and the superior fibres of the adductor brevis, in consequence of the ascent of the head of the femur: the muscles gradually lead to superior activity of growth of the tuberosity of the ischium in the direction in which they pull; persistent traction always has this effect on the nutritive evolution of bone. A very remarkable example of this condition may be seen in the Dupuytren Museum, in the pelvis of an adult female. There are two congenital dislocations; the head and neck of the femur are both wanting, and on both sides the superior extremity of the bone is attached external to the antero-inferior spine of the ileum. The acetabula present a rounded eminence, partially surrounded with a sulcus. The ossa pubis are attenuated, elongated, and less curved from before backwards than usual. The ischia are very prominent externally and anteriorly, and less thick than ordinarily; in consequence of this, the pubic arch is very wide, and not high; it measures four inches and a half between the ischia, and is only about nine lines high. The transverse diameter of the inlet is considerable; that of the inferior immense. But another pelvis, in the same collection, presents still more remarkable deformities: only one femur remains attached to it; the at-

tachment is external to the antero-inferior spine of the left ileum. On the level of the antero-superior spine the right ileum is two inches higher than the left, and the bones are fixed with equal solidity in these two entirely different situations. The sacrum is tolerably broad, but very short; the transverse diameter of the inlet somewhat short, the antero-posterior enlarged. The dimensions of the outlet are enormous in every direction. Changes of this description necessarily influence the process of parturition; some of them prevent its accomplishment, others facilitate it too much.

There are other modifications of the iliac bones met with. The development of this bone is often defective. If there be only one luxation, in ordinary cases one ileum only is atrophous, and the deformity of the inlet, outlet, and pubic arch follows; the latter becomes abnormally straight, and inclines more directly forwards and outwards than natural, as well as the tuberosity of the ischium on the same side. A case of this description is contained in the Museum. Dupuytren, therefore, committed a most serious error in asserting that the phenomena of congenital dislocation exercise no influence on the development of the pelvis, and that that cavity is quite as well adapted for the function of parturition as in the best formed persons. The conditions, to which I have referred, are not mere fortuitous coincidences; they are evidently connected with each other by a link of causality. Again, the ilium is often almost vertical, instead of being inclined, and, as it were, twisted, outwards. This state of things may obstruct gestation; we can understand that it might even interfere with parturition, and that it must always cause obliquity of the uterus on the opposite side of the abdomen. The anterior border of the ilium also presents a singular disposition, a distinct undulation, when looked at in front or even sideways. This is caused by the tendon of the iliacus and psoas, which is attached to the lesser trochanter, being carried upwards out of its natural direction by the ascent of the femur; in consequence of this change of position the tendon excavates more deeply, and alters the direction of, the groove between the ileo-pectineal eminence and the antero inferior spine of the ileum, in which it naturally glides: the peculiarity also depends on the deviation of the last-named process itself.

Congenital dislocations of the femur are also attended with anomalies in *more distant parts*, which are, for the most part, the immediate consequence of the displacement of the head of the femur. These are deviation, shortening, and atrophy of the femur, and of the entire lower extremity; deviation of the vertebral column, which part is also occasionally unnaturally moveable on the pelvis. The lower extremity is often in a permanent state of deviation from *rotation;* in such manner that the point of the toes is turned inwards, as in accidental dislocations on the dorsum of the ilium. Dupuytren affirmed that this was invariably the case; but indubitably he was wrong, for several authors (Paletta, Sédillot, &c.), and I myself have seen the contrary. Yet Dupuytren declares that he had seen more than twenty cases of congenital dislocation; he must either have observed them carelessly, and allowed himself to be guided by a preconceived opinion, or chance must have singularly contributed to mislead him. In fact, it is by no means rare to see the point of the foot turned outwards, or inclined neither to one side or the other; in which latter case the limb has undergone no deviation in its circumference.

The lower extremity is frequently inclined permanently inwards; there is then a tendency to crossing in the two limbs. This mode of displacement is seldom, perhaps never, observed, unless when the point of the foot is turned inwards. In consequence of the rotation of the foot inwards, the patient appears to be affected with the species of club-foot, which I shall term *internal*. The lower extremity on the diseased side is always shorter than that on the healthy, provided the latter be normally developed. This shortening, produced by the ascent of the femur on the os ileum, occasions a swelling of the buttock superiorly, while inferiorly that part is flattened and its fold elevated higher than natural. The shortness of the diseased limb appears to be more marked in adults and old subjects than in very young people; this depends upon the greater length of time the weight of the body has acted on the ileo-femoral capsule and the sound ligament. This, at least, is the doctrine of Dupuytren, and it appears rational enough; nevertheless, the ascent of the limb, and, consequently, its shortening under the influence of the weight of the body, which forces the pelvis and the trunk between the thighs, should apparently cease, once the supplementary acetabulum is surmounted by a bony brim, against which the head of the femur may lean.

Finally, the lower extremity may be more or less atrophous in respect of thickness and length (*Hippocrates, de artic., sec.* 29.), and this atrophy, which commences with the corresponding ileum, extends in a variable manner to the other bones of the member and to the soft parts. When the atrophy is pushed to such an extent that the limb forms only a *light appendix* to the trunk, incapable from the want of proportion between its dimensions and those of the opposite extremity, of serving for progression or support, anchylosis of the joint must supervene in the end.

According to Dupuytren, the vertebral column is thrown backwards in such manner, as to produce a lumbar depression above the nates, deeper than natural, and, consequently, an abnormal prominence of the abdomen.

This peculiarity, which has the effect of carrying a certain quantity of weight backwards, so as to bring the centre and the line of gravity in the same direction, is the result of an instinctive effort, incessantly in action, to maintain the equilibrium in the erect posture. Dupuytren once observed abnormal mobility in the articulation of the spine and sacrum; this must also have been a result of the same species of effort.

*(To be concluded in our next.)*

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ON THE

## ADVANTAGES TO BE DERIVED

FROM THE

## STUDY OF INOCULATION,

IN THE INVESTIGATION AND TREATMENT OF THE

## VENEREAL DISEASE.

*By* WILLIAM ACTON, *Extern to the Female Venereal Hospital.*

(A Paper read before the Parisian Medical Society.)

PRACTITIONERS of the present day, and more particularly those of the French school, may be said, in examining the nature of disease, to employ a method of observation and experiment, joined with strict analysis, which has rarely been applied to the study of medicine.

"We live," says M. Andral, "in an age of transition; we have become too sceptical to place implicit dependence on this or that creed." Loosened, then, from the thraldom of the schools, the present century has produced a class of independant observers who, far from rejecting the recorded testimony of their predecessors, are equally prepared to examine and corroborate their views, or to disprove or interpret afresh the opinions and facts which the great masters of our art have bequeathed us in their immortal works.

*History of Inoculation.*

About the end of the last century, Bell and Hunter performed various experiments. The latter indefatigable surgeon, in his investigations on the nature of the venereal disease, inoculated several persons, and has recorded his experiments. Among others, we find the following:—He took the secretion of venereal sores situated *on the body* of a patient, and inoculated the sound skin with it. No effect ensued. Probably these sores were such as follow secondary symptoms in bad constitutions, and we are not surprised at finding that inoculation did not succeed. He inoculated a patient suffering under venereal pustules with the secretion taken from a primary sore situated on the penis of a recently affected individual; the inoculated points soon presented all the characters of chancres, whereas the venereal pustules got well very rapidly.

A man was inoculated with the secretion of a syphilitic ulcer on the tonsil; no effect resulted. Whereas with the pus of a gonorrhœa, Hunter produced on himself a well-marked chancre, and all the consequences of it, viz., ulceration of the throat, affections of the bones, &c.

Such is a summary of Hunter's experiments. From these he concluded that there was but one virus which might produce either chancre or gonorrhœa, according to the tissues with which it came in contact. He further concluded that the venereal disease was peculiar to man, as he was unable to communicate it to animals notwithstanding all his endeavours.

These were the only conclusions he drew. We shall not stop, therefore, to criticise them, but only remark how singular it is that a mind like Hunter's could have remained satisfied with such meagre deductions.

Hunter's opinions, consequently, had weight, notwithstanding the opposite results obtained by Bell, and more recently by Tod, Harrison, Hernandey, and others, but which we cannot here describe or show how they differ from Hunter, or from one another, as we hasten on to more recent times.

The consequence of so many apparently contradictory experiments was inevitable; the medical world disbelieved them altogether, or placed credence only in those which coincided with the doctrines they themselves professed. Having formed their opinions, surgeons went in search of experiments to support them, and this was not difficult among the mass of contradictory evidence.

This I believe to be a correct view of the state of the question when Mr. Ricord, in 1830, was called upon to fill the situation of surgeon to the Venereal Hospital.

All the experiments performed by his predecessors he repeated before large classes, composed of pupils of all nations, and he last year published a work entitled, *Recherches Critiques et Experimentales sur l'Inoculation appliquée a l'etude des Maladies Veneriennes.* He has given us a view of the experiments which have been performed, and been the interpreter of all, and distinctly shown that although the experiments of others have been well performed, they have been misconstrued.

Previously to entering upon the study of inoculation I shall notice, briefly, the characters of the sore from which the secretion should be taken. Every venereal ulcer indiscriminately does not furnish a secretion which, on inoculation, will produce any peculiar effect; but if an Hunterian chancre be chosen, which is extending itself daily, and has not yet began to furnish granulations, whether that chancre has existed only a few days, or several months, or even

years (for I have seen a chancre which has existed 18 months furnish the inoculable pus); and if it be introduced by means of a lancet underneath the epithelium or epidermis of any part of the body (the inner part of the thigh is usually prefered from convenience), the following results will constantly and inevitably be obtained.

During 24 hours succeeding the operation the inoculated point becomes red; in the course of the second and third days the surrounding parts are slightly swollen, and assume a papular appearance, or already traces of a vesicle are seen in the summit; on the third or fourth day a fluid, which is more or less transparent, is observed beneath the epidermis, and a distinct vesicle becomes apparent, where the papula previously existed, and a dark dot is seen in the centre, owing to the coagulation of the blood which had escaped through the puncture of the lancet; from the fourth to the fifth day the vesicle assumes a pustular character, and a distinct depression is seen in the centre, so that it represents very distinctly at this period the small-pock pustule. The red areola, which has been hitherto gradually augmenting in intensity, now as gradually fades away, and the cellular tissue, which was slightly œdematous, becomes infiltrated with a plastic lymph. On the sixth or seventh day the pustule is observed to be wrinkled, in consequence of the contents becoming thicker, and ultimately a crust takes the place of the pustule. If not interfered with, this crust assumes a conical appearance, increasing always at its base; it ultimately falls off, leaving an ulcer seated on an indurated base, in depth equal to the thickness of the skin: the bottom of the ulcer is covered with a whitish pulpy substance or false membrane, which adheres so firmly, that it is with difficulty wiped or washed off. The ulcer is perfectly circular, and appears as if made with a *punch*, so accurately is the margin dentated, when examined by means of a microscope; the border is slightly indurated, and surrounded with a red areola, giving to the sore a somewhat cup-shaped appearance. The secretion from this ulcer consists of a thin pus, tinged usually with blood, but differing in no perceptible character from the ordinary pus. Examined by chemical tests, it is found to be alkaline. Under the microscope, animalcules may be seen in it, particularly the *vitrio lineola* of Müller; these seem only a coincidence, and are not peculiar to the secretion of chancre, as some authors have supposed.

But it is neither in the physical, chemical, or microscopic characters of the pus, that we have been able to detect anything peculiar; it is in the effects which it produces that we must search for its pathognomonic and characteristic features. Thus, if a small quantity of the pus, from the artificial chancre, be inoculated, the same phenomena, as I have just described, will follow *ad infinitum*, and chancre will thus reproduce a chancre, a property which I have found peculiar to this secretion, and one which I shall hereafter consider will serve us as a valuable diagnostic sign. Another character, which I cannot here pass over in silence, is a disposition such sores have to increase gradually, and in a circular manner, preserving at the same time their peculiar venereal characters. Thus, I have seen these artificial chancres, when left to themselves (a thing greatly to be deprecated), or only treated by simple ointment, become as large as a five-shilling piece; on the contrary, I have seen them, in 24 hours, assume their healthy character, and become covered with granulations, by local treatment alone, and cicatrisation follow, leaving no permanent depression, provided the part affected is plentifully furnished with loose cellular tissue.

The preceding observations, which have been made not only under the eye of Mr. Ricord, but in the Female Venereal Hospital, under Mr. Danyau and Vidal, entitle us to deduce the following axiom:—

That chancre is only inoculable during a peculiar period of its existence, which I may state, from my observation, to be from the third day to eighteen months, and probably longer; during that period the chancre may not cease to secrete the contagious secretion, a very important fact in practice.

But to proceed: if the secretion is brought in contact with the sound skin no effect will follow; there must be a lesion of continuity of surface, so that the contagious principle be absorbed, and however small the abrasion of the skin, whether caused in coition or otherwise, this will take place.

We see at once that neither venereal orgasm, sympathy, erection, &c., are necessary for the introduction of syphilis into the system, a fact which has been often denied by authors.

A point of no little importance is proved by the preceding observations, namely, that no heat or vitality is necessary to cause inoculation to take effect, for I have seen the pus exert its influence when introduced on a steel point; to prove this further, Mr. Ricord has kept the pus seven days in bottles, and the same results have uniformly been obtained.

Inoculation, moreover, proves that there exists a peculiar cause of chancre, which I can thus trace to the secretion of a similar sore. In what part of the pus it exists, I am unable to decide, and, as in many other inquiries, am obliged here to acknowledge our ignorance, as well as of the laws which regulate its secretion.

But if inoculation proves that there is a special cause, it likewise demonstrates that there exists only *one cause* which, under the

circumstances above stated, produces alone one series of phenomena, as constant as they are regular in their effects. Thus, no one in the present day can show the variety of the venereal poisons, as some surgeons have attempted. In a work which I propose publishing, I shall show on what circumstances those supposed various poisons depend.

It follows, as a consequence of inoculation, that the poison of syphilis does not lie dormant in the system to break out on the genital organs. In a few hours after insertion, the pus acts upon the tissues, and a constant development of the pustule follows; it is likewise proved that it is the point where the pus is inserted, which becomes the seat of the chancre, which is thus a local disease, capable of being destroyed totally during the three first days of its existence.

Thus, then, inoculation has been the means of showing the conditions of a chancre, which are necessary for the secretion to become contagious; it has enabled us to prove the *means* of transmission of the veins; it has taught us the necessary conditions of the tissues which are about to be infected; it has enabled us to investigate the nature of the virus, and to see its effects on the animal economy; it has been, moreover, the means of elucidating the question of the nature of many phenomena, which have successfully been reported on, and denied to be venereal. Thus, inoculation has been the means of distinguishing a chancre from all other sores to which the economy is subject; it has proved that no other secretion, whether the result of chronic or acute inflammation is capable of producing similar effects. Hence, syphilis is distinguished from a variety of diseases with which some surgeons have confounded it, and from which other surgeons have separated it, for want of this sure and certain test.

Inoculation has been the means of dividing syphilis into stages; thus it has proved that chancre, at first a local disease, is inoculable; whether it assumes the form of a primitive chancre, or whether absorbed by a lymphatic vessel, it causes suppuration of the ganglion, and consequently a bubo, which may be considered, in reality, an internal chancre enveloped in a ganglion or lymphatic canal.

The second stage is marked by the disease becoming no longer inoculable, notwithstanding the presence of ulceration, inasmuch as inoculation no longer produces the specific effects above spoken of; thus we find that the effects of syphilis differ not only from those of all other poisons, but that the disease has its various periods.

Doctrines so fundamentally opposed as these are to the opinions of the present day, although borne out by facts, necessarily have met with great opposition. Thus the experiments of Hunter, and daily experience, are cited as proof of the inaccuracy of our stating that the secretion of gonorrhœa will not produce chancre.

Instead of disproving our doctrine, the fact which is incontestible, viz., that gonorrhœa will occasionally produce all the consequences which follow chancre, only proves what we have every reason for believing, namely, that chancre occupies the canal of the urethra, and will give rise to all the ordinary symptoms of gonorrhœa, in addition to those of syphilis. That this is not pure speculation on my part, the following facts prove:—B. Bell states that a young man purposely introduced the pus of a chancre into the urethra; a gonorrhœa followed, that is, a discharge of pus, and shortly afterwards a chancre was observed in the urethra.

Mr. Ricord has observed two cases of chancre occupying the whole length of the canal of the urethra, and the pathological conditions of which were shown at the Academy of Medicine, as well as at his clinical lectures, and a drawing of which I possess.

A week scarcely passes at the Venereal Hospital, without persons presenting themselves with chancres at the meatus urinarius.

A second objection has been brought forward against inoculation, namely, it has been stated that secondary symptoms are occasionally inoculable. I must confidently deny that such is the fact; cases like the following, perhaps, have led to this opinion:—

In the bed No. 10, in the second ward of the Venereal Hospital, lies a patient 32 years of age, a shoemaker by trade, of fair complexion. He states that from the age of 13 till his 18th year, he was liable to ulcers on various parts of his body, the cicatrices of which are still visible; from the last mentioned period he has enjoyed good health. About 15 months since a bubo appeared, which suppurated and healed in about six weeks.

About five weeks ago the patient observed a chancre on the prepuce, four days after connection; soon afterwards several chancres appeared around the corona glandis, and he consulted Mr. Ricord as an outpatient. During the time he was following the treatment prescribed, a vesicular eruption made its appearance on the inner part of the left leg, which he distinctly remembers having scratched, and from that moment the sores began, and increased in size.

Present state:—Chancres still exist on the penis, which discharge freely; on the inner part of the left leg there are twelve ulcerations of different sizes, but assuming all the characters of primitive syphilitic sores.* Inoculation with the pus of these

* Mr. Acton, on a subsequent occasion, showed to the Society a plate taken at this

sores was made, and the characteristic pustule was produced.

This, then, instead of being a case of secondary syphilis, which was inoculable, is simply an exema which was inoculated by the nails of the patient, soiled with the secretion of the primitive sore on the penis.

It now remains for me to state in what cases inoculation (employed as we have above stated) should be used, and what advantages we may expect to derive from it. In all cases of obstinate and violent gonorrhœa inoculation should be practised, as we shall be enabled to say if a chancre exists in the canal; we can thus apprise our patient that he is liable to secondary symptoms, and we at once must follow a different line of local treatment, as we have rather to treat a chancre than a gonorrhœa.

Inoculation should be employed in cases of obstinate and suspicious sores seated on any part of the body or mucous membrane. We have given an instance of its value, in a diagnostic point of view, in the preceding pages; to illustrate it further, I subjoin another case from my note book:—

On the 27th of February, 1838, a man presented himself at the out-patient room, complaining of chancre at the *root* of the penis, and accidentally, as it were, showed Mr. Ricord a sore on the frenum of the tongue. The characters of it were sufficient at once to arouse some suspicion as to its nature, and this patient was immediately admitted. Inoculation of the secretion on the sore in the mouth was made, and a characteristic pustule followed. All doubt was now removed, and the avowal of the man additionally proved that this was a primitive syphilitic ulceration, but the further particulars of this case I defer giving at present, reserving it for an article on the syphilitic affections of the mouth and throat.

If I were not afraid of fatiguing your attention, I would detail the case of a woman in the wards of M. Vidal, at the Female Hospital, who presented ulcerations around the rectum, the genital organs being in a healthy state. Here, as in the former case, inoculation proved that the sores were primitive ones, and it was the cause of the avowal of the patient of the manner in which they had been contracted, after every other means had failed of arriving at a confession.

So valuable a means of diagnosis has inoculation become, that Mr. Cullerier, formerly one of the strongest opponents to it, as may be seen by referring to the *Dictionary of Medicine*, states that he would not give an opinion on the syphilitic nature of a sore before a court of law, unless he had inoculated the secretion of it.

Did time allow, or could I trespass on the time of the Society, I would further illustrate the advantages of inoculation, but I am compelled to conclude with a few observations on the abuses of inoculation.

In the first place, I have been a witness of facts which, if they continue, will bring this valuable diagnostic sign into disrepute. Thus the chancre, a consequence of inoculation, instead of being properly treated, is encouraged to extend by ointments composed of various simples; I have seen them of the size of a five shilling piece, and they thus frighten both the surgeon and the patient.*

It is not necessary to inoculate well-marked chancres on the penis which follow connection; not that I believe that any ill consequences will follow an additional chancre (for secondary symptoms bear no relation to the number of chancres which have existed), but because it is superfluous.

Phagedemic chancres should not be inoculated, as the very fact of the sores being phagedemic shows that the patient is in a peculiar state of constitution, and hence all sores will take on the same character, a thing to be avoided, particularly when no good can result. The following case proves this opinion to be correct:—

A patient was admitted into a Parisian Hospital, suffering under various sores on the body; among others, there was a swollen state of the cervical glands, with fistulous openings and ulcers on the neck. On the lower extremities there were several secondary venereal sores, which assumed all the characters of old scorbutic bleeding ulcerations, as hæmorrhage followed on the slightest pressure.

Two of these sores presented every character of primary chancres, masked, however, by the bleeding phagedemic character sometimes seen. In addition to all these ailments, the patient had chancres on the penis, which he had lately contracted. The surgeon here inoculated with the secretion, and chancres of a most unhealthy and intractable nature followed; now, we consider this as a most wilful and unnecessary abuse of inoculation, particularly as the instrument used was *a blunt* bistoury.

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point, which will form one of a series of drawings he proposes publishing to illustrate the venereal disease.

* It is not my intention here to speak of the treatment of chancre, but I may observe that as soon as the characteristic pustule is formed it should be cauterised.

## CLINICAL REMARKS,

*By SIR BENJAMIN C. BRODIE.*

AT

## ST. GEORGE'S HOSPITAL.

(*Continued from page 234.*)

### FEVERS.

*Symptomatic of Local Diseases.*—This species of fevers differs from the idiopathic species, from its presence being dependant upon some revealed or unrevealed form of local disease. I shall not detain your attention, by giving you the history of fevers; but shall point out to you the more important symptoms and changes which these affections produce, modified as they are by local changes of structure in the system. And first of

*Intermittent Symptomatic Fever.*—This is, as you are told in your medical lectures, generally caused by the evolution of marshy miasmata, and is then, of course, quite independent of local disease, and has nothing to do with surgery; but there is a fever very much like it, and that is generally symptomatic of the presence of some local affection. A man had a stricture of the urethra and ague combined with it. He consulted a medical man about the ague, but nothing that he could do would cure him. By and by the stricture became very troublesome, and he came to me for advice about it. I cured the stricture, and the ague left him. Intermittent fever is often accompanied with diseases of the urethra, and it is often present where there are phlegmonous abscesses. The symptoms of symptomatic intermittent fever very frequently resemble those of idiopathic intermittent fever. In each of them you have the regularly progressing symptoms of the hot, cold, and sweating stages. In general, however, the symptomatic intermittent fever has not the same regular periodicity of paroxysm that the idiopathic form possesses, although occasionally we meet with cases where this rule admits of an exception. The type it most frequently assumes is the quotidian. In such a form the paroxysms may occur at any time of the day, and may thus continue regularly recurring for upwards of a month or more. I knew two persons, one of whom had fistula in ano, and the other had abscesses burrowing about the nates, and they both had intermittent fever. In the treatment of these cases you should order the patient to be sent to bed when the cold fit comes on, and give him a dose of Dover's powder immediately, put bottles of warm water to his feet, and an additional blanket on his bed. You may do something towards preventing the supervention of an expected fit, by giving the patient a large dose of opium an hour before the paroxysm is expected. But your great object must be to cure the disease by which the fever is caused. In many cases this is done very easily: if an abscess be the cause you have only to open it; if it be a fistula in ano enlarge the opening. But there are other cases in which this is not so very easy, as, for instance, when the fever is caused by stricture of the urethra. In such a case the patient will be very liable to have a fit whenever the bougie is introduced, and from the frequency of the paroxysms that may thus occur, the patient will become exhausted. But you must remember that the local disease is only one cause of the ague, there must be combined with it a particularly active state of the constitution also. If you can do away with the local cause, so much the better; if not, do what you can towards relieving the patient's constitution, and for this purpose you may give him *arsenic*. Indeed, you must follow the same line of treatment that you would follow in a case of idiopathic fever. I had a woman under my care who had abscess in the neighbourhood of the rectum; she had a paroxysm of ague every afternoon at six o'clock; I gave her *arsenic*, and it relieved her. In general, however, you will find that the peculiar fever which attends upon local diseases, is not the intermittent, but the continued fever; and of this latter form I shall now speak to you.

*Continued Symptomatic Fever.*—There are two varieties of this fever, and one of these has an inflammatory type, which presents the following symptoms:—First there comes on a little chilliness, which in a very short time is succeeded by a hot skin, the pulse becoming quick and hard, the mouth dry, the tongue white and furred, the countenance flushed, the urine high-coloured and scanty, and the bowels costive. The patient is tormented with a great thirst, and desires liquids rather than solids; and all these symptoms become much aggravated towards evening. But there is another variety of this fever, which presents a totally different class of symptoms, more resembling those of typhus. There is pain in the head, the skin is burning hot, the pulse wiry, small, and rapid, the tongue brown and furred, the mouth clammy, great anxiety of countenance, and delirium. As the disease advances the pulse becomes more rapid and feeble, and is obliterated under the slightest pressure; the patient becomes more restless, and turns impatiently from side to side; low delirium comes on; the mouth, the tongue, and the sides of the lips become coated with a black fur. In some other cases of this kind there will be very great determination of blood to the head, and sometimes to the fauces, producing very great difficulty of swallowing, or there will be a congestion of the thoracic viscera, producing difficulty of breathing.

Sometimes, from local diseases, you will find abscess in the structure of the lungs or in the liver, or developed among the serous membranes, although the local diseases causing all this internal mischief, may be far apart from all these viscera, as, for example, in the lower extremities. There are many cases which may at first induce an attack of inflammatory fever, which may be succeeded by one of a typhoid type, just as in idiophatic fever. At other times you will find the typhoid form of fever, the first one which appears, and when it does so, it is owing to the bodily powers of the patient being in a state of extreme debility. Much, however depends on the part which is affected: if fever arises, for instance, from making an incision into the cavity of the tunica vaginalis testis for hydrocele, it would be of an inflammatory kind. In the *treatment* of these cases you must, in the first place, if possible, put a stop to the local disease, and by removing the cause of this you will also benefit the fever. If you consider that your patient will be benefitted by blood-letting, you must regulate the quantity of blood lost according to the sanguineous determination to any part near the local affection. As purgatives are useful in such cases, you may administer the neutral salts, as they tend to increase the secretions from the alimentary canal, and with these you may combine the *spirit of nitric ether*, which will also increase the urinary secretion from the kidneys and diaphoretic medicines, also to increase the exhalent secretions from the skin. In the treatment of the typhoid form of fever, you may, perhaps, have occasion to take blood from the part inflamed, therefore blood-letting may be necessary, effervescing draughts should be given, and the bowels kept open; but you must not purge your patient, for you will find that very mild doses of *calomel* will be quite sufficient for the purpose, and combined with this you should also give your patient cordials to support his strength. When, under these circumstances, typhus fever depends upon sloughing or putrid matter being pent up, you should immediately let this out, for unless you do so you will in vain endeavour to support your patient's strength by internal remedies. There are some cases where the typhoid symptoms are so distinctly marked, that it would be highly imprudent to take away blood locally. In such a case you must attend to the symptomatic fever, and not to the local disease, because the fever is the most dangerous of these two affections, and may soon carry off your patient, if you do not attend to it. When the bodily powers are so much weakened, you must administer stimuli, however extraordinary such treatment may appear, where there is local inflammation, and you must let the local disease take its own course. The symptoms of continued fever generally show themselves when inflammation is present, and before suppuration has come on.

*Hectic Fever.*—This term is applied to that peculiar species of fever which generally depends upon an abscess in a diseased hip-joint, or in an old fracture. It is caused by local irritation, and resembles in many of its peculiar symptoms the last stage of phthisis pulmonalis. In this fever the very least disturbance, as the jarring or jamming of a door, for instance, will manifestly cause an increase in the heart's action. This peculiar fever is known by a frequent but weak pulse, a clean, moist, and white tongue, which becomes sometimes a little furred; the face is frequently flushed; the patient is troubled with diarrhœa and profuse night-perspirations, which leave him in the morning in a state of great debility. A child, for instance, has disease of the hip-joint; hectic fever ensues, as a consequence, with all the train of symptoms which I have enumerated, and when he dies, it is discovered that the general pulmonary structure is in a state of disease.

That particular idiosyncracy of the system which induces hectic fever, is often the means of setting up disease in some other part, especially if it supervenes upon a scrofulous constitution. If you are able to remove the exciting cause, the fever will most probably subside; for instance, if it is found requisite to amputate the diseased limb which caused the fever, it will in all probability gradually subside. Cases will, however, sometimes occur, in which it may be of paramount importance that the limb should be preserved to the patient, if possible; you must then administer *bark, quinine*, or the *diluted mineral acids*. At bed-time you may give the patient a dose of *Dover's powder*, and there is this remarkable fact attending the employment of this remedy in hectic fevers, that it does not increase the perspiration. The patient's diet should be nutritious, and he should take wine combined with other cordials. There are some people who say that hectic fever is the fever of suppuration; they say that it depends upon the absorption of pus, but unfortunately for this theory, it often happens that hectic fever first makes its appearance after the matter has been let out, and not whilst it is pent up, which would not be the case if this assertion were correct.

*Nervous Diseases.*—There are many cases in surgery where the different functions of the frame may be disturbed, and the symptoms marking that disturbance seem to depend upon the nervous system. The series of symptoms which I am now going to detail to you, occurs previous to inflammation, and immediately after the receipt of a local injury. But I shall first offer you a few remarks on a peculiar nervous disease, affect-

ing women more particularly, I mean hysteria.

Hysteria in women is generally caused by a sudden stoppage of the menstrual discharge. It affects different persons in different degrees. Some persons are not hysterical under the same circumstances that others are. In the worst cases of hysteria you will generally find that the patient has a small pulse, cold extremities, numbness of the hands, with a general chilliness of the cutaneous surfaces, arising from a feeble circulation in the capillary vessels, which, in some cases, is so great, that blisters form on the skin. Some hysterical persons will only speak in a whisper, which is owing to the loss of secretion in the larynx. You will be able to find out whether the loss of voice is hysterical or not, from the history of the case, by observing whether there are any other concomitant symptoms in connection with it. When any part of the body has been the subject of nervous pain for any length of time, it will sometimes swell, for instance, a calculus in passing along the ureter into the bladder, causes pain in the testicle of the same side. If the calculus remains for some time in the ureter, the testicle will swell again; or, in some instances, you will find that in some women there will be a nervous pain in the breast, which after lasting for some time, will cause that gland to swell. You will be able to distinguish these anomalous nervous pains from positive inflammation, by inquiring into the state of the patient's constitution, and by the length of time which the pain has lasted. Again you will be consulted by patients, under the supposition that they are the victims of diseased hip-joint, when you will, on inquiry, find that the pain is purely hysterical. In these cases you will generally find a deficient secretion or total want of the menstrual flux; the pain will not be confined to any one particular spot; and if you pinch up the skin, or press upon it with your finger, the patient will almost writhe with severe pain. This state of parts may last for some time without any degeneration of the osseous or ligamentous structures occurring; whereas, if any organic disease of the hip had been present, abscess would too surely have returned. You may also be consulted by women with this same class of anomalous pains in the wrist, elbow and knee-joints. In all these cases you must observe whether the constitution is hysterical or not, or whether the long-lasting pain has produced any abnormal change of structure in the parts affected. There are many young women who are supposed to have caries of the spine, but which after all is only a local hysterical affection. This will be clearly diagnosticated by you if you follow out the rules which I have previously laid down for your guidance in all these peculiar affections, in addition to which you will find that in these latter cases the weakness is totally different from that of real caries of the spine. However, in the incipient cases of caries, there is scarcely any symptom whereby you can certainly say whether the disease is caries or not. Three months cannot pass over, however, before you will be well able to ascertain for yourselves, and when the disease is established, take care not to mistake it, because in these the treatment will require all the judgment and skill you are masters of. Cases of pure hysteria have been treated as pure caries of the spine; caustic issues have been made, and the patient has been placed in the horizontal position for months. Now, observe the mischief that ensues when a case has been thus maltreated; the patient has not only been placed and kept in this unpleasant posture, but she will find the greatest difficulty in walking again, and may perhaps never be able to do it well. Again, a woman watches over a sick child for some days together; she feels pain in the hand, which she attributes to injury; the pain extends to the arm; by and by a puffy swelling appears on the back of the hand, which swelling, after a little time, goes off, leaving the parts glossy, tense, and extremely painful; she loses the use of her hand for a few days; but in almost all these cases recovery follows with certainty. Sometimes also, in these cases, you have a spasmodic constriction of the muscles. The same thing may happen in the lower extremities, and you will know there is no deception, as the parts are almost always colder than natural. The symptoms I have now given you will furnish you with some idea of the nature of these cases. There are, however, one or two remarkable symptoms in these hysterical diseases, which I have not spoken of yet. One of these is an inability to pass water. In these cases you will be obliged to introduce the catheter, but you must be careful never to introduce it oftener than is absolutely necessary. There is another symptom which I have occasionally met with, which is a crackling of the joints, as though the cartilages were rubbing against each other. I can only account for this by the action of the muscles upon the joint.

There is great sympathy existing between corporeal and mental diseases, and this is very evident in hysteria. From the sudden stoppage of menses from fever or cold, the mind often becomes so much affected that insanity is produced.

*Treatment.*—On this head I must first observe to you again on the danger of treating hysterical diseases as you would other diseases. Another object which you must have in view is to withdraw the patient's attention as much as possible from the disease. This alone will do more good than anything else. In some cases it is better to treat them by doing nothing at all, because by employing

local applications you draw the patient's attention to the seat of the affection. Where there is much pain in the part you may apply a belladonna plaister over it, or you may moisten it with equal parts of tepid rose water and camphor mixture, and you will find that this application will really relieve the pain. More good however is to be done in these cases by constitutional remedies. If the menstrual discharge has stopped, give steel and myrrh. If the circulation is feeble it will get stronger by giving the carbonate of ammonia in small and repeated doses, either in camphor mixture or in pills made with crumb of bread. If you give it in the latter form, of course the pills must not be put into boxes, but into a well-stoppeled vial. The following is a very good recipe for hysterical pains:—

*Tincture of ammoniated iron*, half a drachm;
*Carbonate of ammonia*, five grains;
*Camphor mixture*, one ounce and a half.
To be taken three times daily.

You must let your patient take exercise, and ride out on horseback, if possible. A person may have pain in the arm after bleeding, because it is said that the nerve is injured, but in the great majority of cases it arises purely from an hysterical constitution. I will give you one or two cases to show you that hysterical disease depends upon the constitution. I know a lady, who whenever she hurts her knee, has very violent nervous pain in it, with spasmodic flexion of the leg or thigh. Such symptoms may in some cases be even so violent as to occasion death. A person injured a nerve in the finger, and there was a constant pain in it whenever the part was moved; in a short time the pain passed up the neck, and the patient went into a perfect stupor, for the relief of which she was bled and cupped with great benefit. In a few days another attack came on, in which she died. On examining the body after death, nothing was found; nothing abnormal could be detected, either in the brain or nervous system; but when I looked to the bladder I found it was distended with a large volume of dark-coloured urine, and I do not doubt that this patient laboured under an hysterical suppression of urine. This was the only morbid appearance I could detect, and I believe that this was the cause of death.

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## RECOVERY AFTER HANGING,

BY

## MEANS OF BLEEDING AND AMMONIA LINIMENT.

*To the Editor of* THE LANCET.

SIR:—The following case may possess sufficient interest for insertion in your valuable periodical. I am, Sir, your obedient servant,

JOHN JOHNSTON, Surgeon.
15, Marylebone-street, Regent-street,
12th November, 1839.

On Sunday, the 27th ult., I was hastily summoned to attend upon a woman who had attempted suicide by hanging herself, and, as the house where the attempt had been made was in the immediate neighbourhood, I was present in about ten minutes after the person had been cut down. I found the woman in a state of complete insensibility; her respiration was laboured and slow, and the pulse barely perceptible. Without further examination of the symptoms, I at once proceeded to open one of the veins of the arm. The blood flowed pretty freely, and as it escaped, the pulse rapidly improved in strength and character, and the respiration also became less laboured. After about 16 ounces of blood had been taken, with the concurrence of Mr. Wise, a medical man in the neighbourhood, who by this time had joined me, I thought it advisable to wait its further effects, and in the mean time a strong turpentine enema was administered, mustard sinapisms were applied to the calfs of the legs, a bottle of hot water to the feet, and cold application to the head; the stays were also loosened, and the patient was placed in the most favourable position for the performance of respiration. We had now a little leisure to examine more minutely the symptoms and appearances which the patient presented. The countenance was not turgid, or livid, but pale; the under jaw considerably fallen, as in a person moribund; the extremities were moderately warm, and the hands convulsively clenched; the pupils somewhat dilated, and barely susceptible to the stimulus of light. A dusky red mark, of about a quarter of an inch in breadth, was distinctly observed encircling the upper part of the neck, forming an angle over the ramus of the jaw on the right side, where the knot of the noose (formed by a silk handkerchief) had rested, and in consequence of this circumstance, the strangulation proved less effectual.

After waiting for about half an hour, and finding that, although the pulse continued good, and the respiration comparatively free, there was no appearance of returning sensibility, we resorted to a second abstraction of blood, and from the same opening 16 ounces were taken, when the pulse became quick, and slightly intermittent. A change for the worse now took place in the state of the patient, probably from the bleeding being carried a little too far. The respiration became loudly stertorous, the pupils dilated to their fullest extent, and completely insensible to the stimulus of light; the lower jaw fell further, and the sphincters becoming relaxed, the contents of the rectum and

bladder were evacuated, and, to all appearance, further measures seemed useless, and the patient's doom seemed sealed.

Under these circumstances, as a last resource, we proceeded to rub the chest and upper extremities with a strong liniment of ammonia, and in a short time our perseverance was rewarded by a decided and favourable change—the woman began to draw up her limbs, as if sensible of the pain of the applications; the pulse regained its strength and regularity, while the respiration lost its stertor, and became regular and less laboured, and presently our patient begun to utter loud moans, indicative of extreme anguish. We continued the friction with the liniment, and the application of the sinapisms, until the cries of the patient became truly painful, and the tossing of the limbs evinced that she was acutely sensible to the pain which these applications caused.

From this time our patient continued gradually to improve, the pupils resuming their natural appearance and sensibility, and in the course of an hour she was able to swallow a draught containing two drops of croton oil, but which she shortly afterwards vomited. Although conscious of pain, she continued in a comatose state until the evening, when she became perfectly sensible to surrounding objects. By this time she had been removed from the apartment where she had suspended herself, to her own room, and as she betrayed by her movements, and the wildness of expression of countenance, an inclination to make a second attempt upon her life, strict orders were given that she should be constantly watched, the hair was also cut short, and cold lotion kept constantly applied.

On the next day I found her more composed; she complained of nothing but *her throat*, and said that her head had ached badly during the night, but she did not *then* complain of any head-ache. From her manner I perceived that she was aware of what she had been guilty, but wished to appear unconscious of what had occurred; and, thinking it better to be silent on the subject, I was thus prevented from questioning her regarding her sensations when suspended, and during returning sensibility.

From this period she remained under the charge of Mr. Wise, who was the medical man of the family in whose service she was a housekeeper; the necessity of keeping a strict watch over her movements continued, but in the course of a week she was sufficiently well to admit of being removed from her place, under the charge of a relation, on the 4th instant. She had not, however, previously confessed that she was at all aware of the attempt she had made upon her life.

It appears that this poor woman had once been in circumstances that placed her above dependance on service. To drown the recollection of this, she gave herself up to tippling, and, in consequence, had received warning to quit her situation. Her threat of making away with herself had not been regarded; but when, on the Sunday, she had been missed for about ten minutes, a search was made for her, and she was found suspended to one of the cross-bars of a four-posted bedstead, and, probably, would have succeeded in effecting her purpose, had it not been for the clumsy manner in which, fortunately, she had adapted the noose.

I ascertained that she was slightly inebriated before she was missed. She was about 33 years of age, and of a robust appearance.

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## REMOVAL OF A TUMOUR FROM THE ANTRUM.

*To the Editor of* THE LANCET.

SIR:—The insertion of the following case in your valuable journal, will oblige, yours respectfully,

WM. QUINTON, Surgeon.

Wolverhampton, Nov. 20, 1839.

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Emanuel Blake, aged 17, on the 20th of October applied to me, stating that he had a large gum-boil on the upper jaw, which, for the previous three months, had been very painful and troublesome, so much so that he could not masticate on that side. On examining his mouth, I perceived a tumour protruding from the left antrum, rather firm, elastic, of the consistence and appearance of the gum, irregular on the surface, and about the size of a moderate hen egg. Knowing the disease to be of a malignant character, I stated to the lad the necessity of an operation, to which he acceeded. I then made arrangements for the same, and undertook the care of the boy at my house until he should perfectly recover.

OPERATION.

Nov. 4. The boy sitting upon a chair, and the last incisor tooth being extracted, I commenced by making an incision with a strong sharp-pointed bistoury, extending from the external angular process of the frontal bone, to the angle of the mouth. I then reflected the flaps upwards and downwards; this being done, the tumour was brought fully into view. With Liston's sharp-pointed bone-nippers, taking the course of the extracted tooth, I cut up to the floor of the orbit; then crossing immediately below the orbital plate, and removing a portion of the malar bone, I extended the incision to near the zygomatic fossa. I then cut up in front of the last molar tooth to the before-mentioned cross incision, and the carotid artery being compressed by an assistant, I firmly grasped the detached portion of the bone, including the tumour. I tore it down, and the remaining attachments being divided

by the knife, I carefully preserved the velum palati, and also the palatine plate of the palate bone. The loss of blood was not great, and not a single vessel required a ligature. An hour after the operation, I placed a pledget of lint dipped in olive oil in the internal wound, and the external was then brought together by three interrupted sutures; he was then placed in bed. Two hours afterwards an anodyne was given, and there being no hæmorrhage, the wound between the several sutures was dressed with isinglass plaister. The lad was comfortable during the remainder of the day.

5. Slept well during the night, and complains very little of pain; skin natural; pulse 96; tongue clean, and able to take broth or gruel. Ordered 2 grains of calomel, half a grain of tartarised antimony, and 10 grains of compound jalap powder, immediately. 5 o'clock, P.M. Bowels not moved; skin rather hot; pulse 100. Repeat the powder. 10 o'clock. Complains of feeling rather sick; skin hot; tongue rather dry; pulse 93. The bowels having not yet operated, ordered 6 drachms of castor oil, to be taken immediately. Removed two of the sutures, and supplied their place with strips of plaister.

6. Skin natural; tongue rather furred; pulse 90; bowels evacuated three times during the night; complains of thirst. Ordered the following draught:—of spirits of nitre half a drachm, of ipecacuanha wine 12 min., of camphor mixture one ounce, directly; to drink barley water or broth. Removed other suture at the angle of the mouth.

7. To my astonishment, this morning the boy was dressed and sitting up, and requested my permission to go down stairs, to which, of course, I did not accede.

8. Going on well.

9. Going on well. Removed the plaisters, the external wound having completely healed by the first intention.

18. Perfectly recovered.

## COUGH FROM ELONGATED UVULA. THE BILE.

*To the Editor of* THE LANCET.

SIR:—I take the liberty of trespassing on your columns by a few observations on the subject of two papers in the last volume of the transactions of the *Provincial Medical Association*; one on an affection of the uvula, by Mr. Thomson, surgeon to the Whitby Fever Infirmary; the other by Dr. M'Cabe, of Cheltenham, on disease of the liver.

I believe that in a very large number of cases of cough the ailment extends no further than the uvula, the elongation of which occasions an irritation of the fauces, and produces this troublesome symptom, the contents of the chest being free from disease only inasmuch as they are secondarily excited to action, and therefore liable to organic lesion ultimately. Recollecting that the upper division of the head, in which the uvula is situated, sympathises all through by membranous communication, &c., directly, it follows that counter-irritation, at a central and opposite point of the upper division, is adapted to the case. This view, during much experience, has led me to apply a moderate-sized blister to the neck, and in every instance successfully, when the ailment had not been of long standing, and purgation had been attended to. Let any one use a few minutes rough friction on the upper posterior part of the neck, and the consent with the interior will be manifest. Blistering, as usually practised on the forepart, does not influence the upper division, and shows a want of discrimination, which is too often to be lamented.

Dr. M'Cabe invites attention to the function of the liver, with reference to the fluid that it secretes, which he thinks ought to be considered rather as designed to be ejected, like the urine and perspirable matter, than retained for the purpose of promoting digestion. The reasoning in support of his views is judicious, and his desire that the point should be discussed, is praiseworthy. We know that out of its proper channel bile is highly deleterious, and acts as a poison. Indeed, its extreme acrimony would lead to *à priori* reasoning against its being retained, until the slow and gentle process of digestion was effected. The subsidence of violent symptoms, on its being gradually and constantly removed, strongly favours the opinion that the liver is an emunctory merely, and not designed to act a part in assimilation, but rather in the protrusion of fæcal matter. We ought ever to keep in view, that every fluid is excreted except blood, which is retained to regenerate, sustain, and enable every part to perform its appropriate duty. Thus, it is expended, but never wasted, unless by the injudicious practitioner, who examines not the designs of nature, but blindly pursues a routine course, without chart or compass.—I am, Sir, yours,

AN EXPERIENCED PRACTITIONER.

Cheltenham, Nov. 16, 1839.

## OCCURRENCE OF SMALL-POX TWICE.

*To the Editor of* THE LANCET.

SIR:—In a letter which I had the pleasure to address to you a short time since, in reply to one which appeared in THE LANCET, from Mr. Marshall, on the subject of the recurrence of small-pox, and which circumstances

prevented you, at the time, from inserting, I stated certain facts, which, to my mind, were most important, being the results of 896 cases of small-pox, not exhibiting one instance of a secondary attack.

I imagined that Mr. Marshall misunderstood my meaning, and I endeavoured to set myself right in his opinion. I assented to his position, that small-pox might, and does, attack the same person twice; but I doubted the frequency of such recurrence, and I gave a statement of 896 cases of small-pox, in which there did *not* occur one instance of a second attack, to place by the side of Mr. Marshall's 433 cases, of which 10 were cases in which he says it *did* occur twice.

If you will allow me, I will again state the particulars of these 896 cases. In the year 1838, 712 patients were admitted to the Small-pox Hospital.

396 were entirely unprotected either by vaccination or inoculation;
298 had been vaccinated;
18 were not cases of small-pox, although sent to that institution under the belief that they were.
712

In Norwich, in 1819, Mr. Cross examined into the particulars of 603 persons exposed to small-pox:—

215 were unprotected; 200 had small-pox; 15 escaped.
91 had been vaccinated; 2 had small-pox; 89 escaped.
297 had previously had small-pox; all escaped.
603

Thus we have 712 cases at the Small-pox Hospital, and 202 in Norwich, making 914, from which deduct 18, which were not small-pox, and that will leave 896 cases of small-pox, not exhibiting one instance of small-pox after small-pox. From these facts I am led to doubt the accuracy of the information upon which Mr. Marshall has drawn his conclusions, and I am induced to believe that if small-pox after small-pox occurs so frequently, as nearly 2½ per cent., the statistical accounts of the Small-pox Hospital would afford a material testimony in support of such proposition. There can be no doubt that the sources of error are numerous. Dr. Gregory says that he has seen secondary syphilis, febrile lichen, porrigo favosa, and varicella, all mistaken for small-pox. Last year the fever which was so prevalent, called "spotted fever," was in many instances supposed to be small-pox at the onset; and I believe some of the 18 cases sent to the Small-pox Hospital were of that character. With respect to these 18 cases, suppose they had all supervened upon small-pox, how very much would they have led to error, supposing they had been cited as examples of small-pox after small-pox. Suppose, at any future period, these 18 people should really become the subjects of small-pox, their having been in the Small-pox Hospital will sanction the belief that they laboured under that disease when there, and the medical man will be led into the error of imagining he is treating a case of small-pox attacking the same person *thrice;* or, if all these 18 people had been vaccinated, and at a future period they should be attacked by small-pox, the mistake of sending them to the Small-pox Hospital will lead to the supposition that they are labouring under small-pox a *second time after vaccination.*

Mr. Marshall says, that of the 433 cases, some were attended by himself, and some by *illegal practitioners.* I am sure he will excuse me for suspecting the validity of evidence derived from *illegal practitioners*, and also my astonishment at the immense proportion of secondary cases of small-pox, when I cannot find any corroborative testimony in support of its accuracy. In THE LANCET of the 16th instant, Mr. Marshall inserts an extract of a letter from Mr. Jeffry, of Sidmouth, relating three cases of small-pox after small-pox, "during the last epidemic," and one case by Mr. Parkin, of the Royal Marines. Mr. Marshall will pardon my saying he is labouring to prove that which has not been denied, and sedulously abstains from quoting that which I require to convince me of his position—the concurrent testimony of figures and proportion. With "Mr. Hunter, of Budleigh Salterton," who has *seen small pox in the same individual four times*, I cannot deal at all.

This subject is so interesting, and the truth so desirable, that I think it would be important that cases professing to be examples of secondary small-pox should be communicated to the profession, in order that they might undergo the test of scrutiny, and so set the truth on indisputable grounds.—I am, Sir, your

Nov. 20th, 1839. CONSTANT READER.

* As "Constant Reader" is desirous of obtaining information on the occurrence of small-pox twice, we offer the following facts for his consideration:—

In the Würtumberg epidemic 631 persons were attacked by *true* small-pox; of these 39 had been affected by *true* small-pox at some previous period, being a proportion of 1 in 16. The nature of the first attack was determined, partly from the evident traces of confluent small-pox on the body and partly from the testimony of the medical men by whom the patients had been attended. Fourteen of the 39 fell victims to the second attack. In the epidemic described by Möhl, 153 out of 988 persons had a second attack, of small-pox; 31 of the 153 died; here, however, the evidence of the first attack is far from being so precise as during the Würtemberg epidemic.

## CONSTRICTION OF THE JEJUNUM BY A CONGENITAL PROLONGATION OF ITS COATS.

*By* WILLIAM MAJOR, *Esq., Surgeon.*

On Friday morning, Nov. 8, 1839, nine o'clock A.M., I was requested to see Mrs. A., York-street, aged 37, of a spare habit of body and sanguineous temperament, who complained of sickness and an obtuse pain over the umbilical region, which had existed more or less at intervals during the ten or twelve previous days; pain was unaffected by pressure and unaccompanied by pyrexia. The bowels had acted moderately on the previous day. Calomel and opium were given directly, followed by a cathartic draught every two hours. 3 P.M. Medicine being rejected, fomentations to be applied over the abdomen; colocynth and calomel pills immediately. 9 P.M. Bowels have not acted, sickness unabated, pain aggravated and attended with slight tenderness on pressure, and felt also in the left lumbar region, where, from the spasmodic contraction of the corresponding muscular fibres, a hard and pretty defined prominence existed, which subsequently was lost, but others similar appeared in various different situations; abstracted ten ounces of blood from the arm; fomentations continued; the pills repeated every hour, with the addition of croton oil, and an enema was administered, composed of turpentine, sulphate of magnesia, opium and gruel.

Nov. 9, ten A.M. Symptoms slightly relieved, restless night, little urine voided; bowels still unopened, and spasmodic pains unabated. A tea spoonful of drops, consisting of opium, ether, and camphor, every twenty minutes, and enema repeated. 4 P.M. No improvement, and stomach incapable of retaining anything; a liniment consisting of croton oil, olive oil, camphor, and opium, was rubbed over the abdomen. 10 P.M. Pain much relieved, and sickness abated; repeat the liniment and an ounce of castor oil every two hours, of which four doses were retained (the castor oil was not prescribed by me but given by the nurse).

Nov. 10, eight A.M. Restless night, bowels still unrelieved, pain more urgent, and increased considerably on pressure; leeches applied to the abdomen, and followed by fomentations; the enema and castor oil (in the form of emulsion) repeated. 4 P.M. Pain relieved, bowels not opened, sickness urgent; repeat the enema, omitting the turpentine; a blister to the abdomen.

Nov. 11, ten A.M. Last night the enema was soon followed by a copious evacuation of feculent matter from the bowels; pain has ceased, and she has slept four or five hours; the strength this morning is evidently diminished; pulse 120, and considerable thirst; ordered saline camphor mixture. 6 P.M. Pain returned; considerable distension of the abdomen, and vomiting of dark grumous matter; fomentations continued, and an anodyne enema to be injected.

Nov. 12, six A.M. A fœtus (apparently five months) was expelled with scarcely any perceptible uterine action, the placenta of which, having been retained some hours, was readily removed by introduction of the hand; after which brandy was given, as the patient appeared to be sinking, but without effect; for, from the expulsion of the uterine contents, the vomiting was incessant, and the strength rapidly declining, was exhausted at 8 P.M.

*Post mortem examination.* — Anxious to learn the cause of the obstruction, I examined the abdominal contents, 20 hours after death, with Mr. Halford, when the stomach was found considerably distended with flatus and dark grumous matter, the intestines throughout presented no evidence of disease, excepting about the commencement of the jejunum, which was full of blood, and of a dark purple colour, which latter state extended from about the last third of the duodenum to three or four inches down the jejunum, at which point the latter was surrounded by a distinct cord-like prolongation, which arose from the anterior surface of the portion of the jejunum beyond the ligature, whence it passed downwards in front of the gut, then coiled beneath, through an opening in the mesentery, and ascended behind it so as to reach the point from which it commenced, whence it again descended between its origin and the gut, thus completing a perfect knot or ligature around it, incapable of being untied, from the circumstance of there being a triangular tubercle attached to the extremity of the cord, too large to admit by any possibility of its being thrust back between its neck and the gut.

The cord-like prolongation, on careful examination, was found to be hollow, its cavity communicating with the interior of the gut at its commencement, and with the centre of the tubercle (which is slightly excavated and apparently lined with mucous membrane) at its termination.

Further, this cord appeared to be a distinct continuation of the jejunum, as it was found to be composed of the three intestinal tunics, and as valvulæ conniventes were distinctly visible on its interior for some short distance after its origin.

The three coats were also distinct in the tubercle in which the cord terminated, the bulk of the tubercle being apparently composed of the muscular coat in a state of hypertrophy.

P.S. The preparation is now in the possession of Mr. Luke, who will be pleased to allow those interested in the case to examine it, on application to the Museum, London Hospital.

12, Mount-place, Whitechapel, Nov. 18.

## OUTLINES
OF A CASE OF
## *ACUTE ANASARCA & CONVULSION*
AFTER SCARLATINA,

By MARSHALL HALL, M.D., F.R.S., &c.

*To the Editor of* THE LANCET.

SIR:—I am indebted to Mr. Duffin for the opportunity of witnessing one of the most deeply interesting cases it has ever been my lot to treat.

A year and a half ago Mr. Duffin took me to Highgate; we met Mr. Snow. The patient was a boy aged 12. Sixteen days before he had gone through scarlatina in its very mildest form; he had scarcely been confined to bed, and had not suffered from the *nimia medici diligentia;* he had appeared quite well.

On this Sunday morning he was seized with swelling of the face, which came on and increased equally suddenly and rapidly. With this symptom there was the appearance of sudden and serious collapse, and soon afterwards convulsions followed by coma.

When I first saw my patient there were convulsions followed by deep coma; wine and brandy on the table indicated sufficiently the previous state of the case. I felt persuaded, in spite of these appearances, that the only hope was afforded by relieving the vascular system within the head, and yet that the measure was not unattended with danger. This view was freely explained to the boy's father, who very sensibly said he confided his son's life to the hands of his medical advisers.

We placed the patient upright, and Mr. Duffin opened the jugular vein. I kept my finger on the pulse whilst we allowed *twenty ounces* of blood to flow! The convulsions ceased, and the coma diminished, but did not disappear. We then ventured to open a vein in the arm, and abstracted seven more ounces of blood!

In less than an hour the little patient knew his parents. We prescribed calomel and purgative medicine; a cold lotion to the head, and fomentations to the feet; afterwards leeches were applied; but the bloodletting was *the* remedy to which the amendment was obviously due. The little boy recovered forthwith, and what is important, without the least symptom of the morbid effects of loss of blood.

I must here remark, 1st, that acute anasarca and convulsions may, and frequently do, follow the mildest cases of scarlatina, perhaps because, after *such* cases, less precaution is usually taken to clear the bowels; 2nd, that in all cases of acute anasarca there is the danger of an affection of the head, arachnitis, coma, or convulsion; and 3rd, that in such cases the remedy is blood-letting—*blood-letting until relief and security are obtained.*

I have thought it right to send this case for publication, in order that I might insist upon the importance and necessity of an energetic use of this powerful remedy in such fearful circumstances. On another occasion I purpose to send you some remarks on the treatment of convulsions generally, both during the interval and during the paroxysm.—I am, Sir, yours, &c.,

MARSHALL HALL.

14, Manchester-square,
Nov. 18, 1839.

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## PHYSIOLOGY OF MUSCULAR ACTION.

*To the Editor of* THE LANCET.

SIR:—I hasten to reply to the letter of Dr. Carpenter, published in your Journal of last Saturday, and I assure you that I do not come forward reluctantly to do him that justice which he has a right to expect. Several of my medical friends are well aware that, before his letter appeared in THE LANCET, I was desirous of expressing my regret at having underrated the merit of his Thesis.

I freely acknowledge that Dr. Carpenter has derived his information from many other sources besides Leuret; that his Thesis is *much more* than a mere compilation; and that I erred in supposing he had not assigned reasons for his adhesion to Dr. M. Hall's opinion "regarding the distinctness of the excito-motor from the sensorio-volitional system of nerves." Having underrated his Thesis, I feel pleasure in stating (although this statement is not absolutely necessary) that I believe his Thesis gives by far the clearest and most connected account of the nervous system to be found in the English, or even the French and German languages. I may observe that my mistake as to Dr. Carpenter not having put forward arguments in favour of the view, for which he has abandoned his former opinion, partly arose from observing that, in his introduction, he had referred the reader, for information on this point, to the general summary, without naming any other portion of his essay.

I am perfectly aware that Dr. Carpenter's essay of 1837, was entitled "On the Voluntary and Instinctive Actions of Living Beings."—(*Edin. Med. and Surg. Journ.*, vol. 48, p. 22.) A reference to this paper will, however, prove that Dr. Carpenter did not advance incidentally or doubtingly his opinion regarding voluntary motion; a considerable space is devoted to an explanation of his opinions on this point, and, moreover, the following is the 12th article of his general summary:—

"As all the nerves of sensation terminate ultimately in the cerebro-spinal axis (the

spinal cord and its prolongations as far as the crura cerebri and corpora quadrigemena), and all the nerves of motion arise from it, it follows that all motions must be the result of some stimulus applied to this system, and this stimulus may be either given by an impression from without or by a mental change within (or in the brain above.) If we regard the cineritious matter of the brain as a seat of consciousness, sensation, the intellectual powers, and volition, the medullary part of the hemispheric ganglia being merely a conductor, (a position which I do not wish to be understood as advocating), it necessarily follows from the position previously taken, that the sensory and motor tracts in the brain simply convey upwards to the cineritious matter the influence of the impressions existing in the cerebro-spinal axis, and downwards the motive action resulting either immediately from sensation or volition."—(*Edin. Med. and Surg. Journ.*, vol. 48, pp. 43 and 44.)

The reasons given by Dr. Carpenter for his adhesion to Dr. Marshall Hall's opinion regarding the distinctness of the excito-motor from the sensorio-volitional tract, do not appear satisfactory to my mind; I think they do not warrant him in repudiating his former opinion. There can be no doubt, however, that this subject will engage the attention of men of talent who can devote much more time to physiology and comparative anatomy than I can. I have done nothing but open a debate in which, I trust, many will take part, and I shall endeavour to take an unprejudiced view of the arguments that may be adduced.

If Dr. Carpenter will look into my paper, and examine the context, he will find that, in speaking of what had been done by Dr. Hall and others, before the publication of his essay of 1837, I alluded solely to experiments on animals.

With regard to Dr. Carpenter's claim of priority, the necessary *originality* of prize essays and other matters, I shall say nothing, for various reasons,—1st. Dr. Carpenter has asserted his claims at considerable length in the same Journal in which my paper appeared; 2nd. A portion of his communication is apparently addressed to Dr. M. Hall himself, and consequently any remarks of mine would be an intrusion; and 3dly. Because I am unwilling to trespass unnecessarily upon your space. I have the honour to be, Sir, your obedient servant,

MARTIN H. LYNCH.

Queen-square, Newcastle,
19th Nov. 1839.

## THE ORGAN OF THE LOVE OF LIFE.

*To the Editor of* THE LANCET.

SIR:—It is often the case that men misspend their time in seeking in remote causes for the origination of phenomena, when an efficient explanation lies within their immediate grasp. This is peculiarly the case with respect to mental phenomena—men search for causes in tortuous speculations, when a just observation would afford a speedy and certain solution of the problem. This is illustrated by the discussion at the Westminster Medical Society, on the "Cause of Suicide," which, however interesting from the facts elicited, occasions regret that the doctrines of phrenology were not brought to bear upon the consideration of a subject, which, from its cerebral origin, lies peculiarly within its province.

Dr. Andrew Combe was the first writer who threw any light upon the subject, or, rather, who first preferred observation to conjecture. It appears, when he examined the brain of a lady, in whom a love of life was the chief characteristic, that he noticed (to use his own words) "an immense development of one convolution at the base of the middle lobe of the brain; it was that lying towards the mesial line on the basilar and inner sides of the middle lobe, and consequently of destructiveness." As this organ cannot, from its situation, be demonstrated during life, dissection can alone prove whether it has any existence; and, as the fact related by Dr. Combe is supported by confirmatory facts, I consider that his opinion deserves further investigation.

There can be no doubt that some persons manifest a love of life with the energy of a primitive impulse, whilst others appear almost devoid of the feeling. Dr. Johnson may be noticed as a person who manifested "Love of life to an extraordinary extent:" in him it became a virulent disease, embittering his fairest prospects, and making life miserable through his morbid fear of death. This was the case before he became a successful author; and even when reduced, with his friend Savage, to the greatest extremities, he still clung to life with the tenacity of a drowning man. The case of Dr. Johnson is not, however, a solitary one, nor is the reverse infrequent. Cowper attempted self-murder several times, although his private circumstances were altogether favourable. Considering, therefore, that love of life is not merely the result of satisfaction with present things, but that it can exist vividly when all is black and lowering, and be deficient when prosperity clothes the individual with its smiling dower, I think that love of life is the result of a primitive faculty.—I am, Sir, yours truly,

November, 1839. E. J. HYTCHE.

## THE DUBLIN LYING-IN HOSPITAL.

*To the Editor of* THE LANCET.

SIR:—Perhaps the following sketch of one of the most useful of our institutions may prove acceptable to some of the many readers of your valuable periodical.

The Dublin Lying-in Hospital was founded in 1756, by Dr. Bart. Mosse, and contains 13 wards, possessing in all 140 beds; of these, 10 wards are laid out for delivery cases, and are termed "labour wards." Nos. 6 and 9 are *recovery* wards, and No. 13 contains 12 beds for *surgical diseases* consequent on pregnancy.

In the *labour* wards the beds are filled in rotation; thus cases are taken into No. 1, until the beds are occupied, and then No. 2 commences to be filled, and so on; the patients remaining in hospital until fit to be discharged, which is usually about the eighth day; after which the ward undergoes a thorough cleansing, by scouring, and the floors are sprinkled with chloride lime, the blankets being suspended in the chlorine atmosphere; after this operation, the ward is generally vacant for a couple of days.

Wards 6 and 9 are occupied with cases where disease appears subsequent to delivery, or where the recovery may be unusually tedious.

Ward 13 contains the most interesting cases in hospital. Here polypus, inversion, and prolapsus uteri may be seen, with vesico and recto-vaginal fistulæ, cancer, amenorrhœa, &c., with the numerous other diseases incident to the parturient female. This ward I believe to be a most valuable addition to Mosse's foundation, and Dr. Kennedy, the present master, has also in contemplation the forming a ward for diseases of children.*

Having briefly mentioned the extent of the institution, I shall give an outline of the manner in which business is conducted, and in this place too much praise cannot be bestowed upon the happy arrangement and distribution of labour, which reigns with undoubted success in every office of the house.

The institution is directed by the master, Dr. Evory Kennedy, and two assistants, Drs. Herdman and Isdell, all resident; the two latter performing the office of the former in his absence, and keeping a statistical report of the cases in hospital. At 10 o'clock A.M., Dr. Kennedy visits the wards in succession, accompanied by his class, and in ordinary cases goes through the usual routine of questions. While passing on, he usually pitches on some case, and questions the class as to its nature and treatment; by this means keeping their memory on the stretch, measures the extent of their knowledge, makes them attentive to the cases before them, and serves to stimulate them to the attainment of further information. If any instruction deserves the name of *clinical* it is this; and if it be sought for in its full vigour, it may be found here. The hospital is visited every evening, also, by Dr. K., or the assistants, and in the interim, should any case require assistance, it is immediately afforded, such as the application of the lever, forceps, perforator, &c. There is also a valuable museum, the property of Dr. Kennedy, containing a number of spirit preparations illustrative of every subject in midwifery, with copious collections of drawings from nature, many of them the produce of his own pencil, and when these are exhibited to his class they are accompanied by the original preparation; there is also a collection of casts in plaster of Paris, taken from hospital cases.

Dr. Kennedy delivers four courses of lectures annually; and, as his style is unique, perpaps it may not be amiss to give a faint outline of it.

He is a man rather above the middle size, and in prime of life. He combines in his oral delivery the air of a master with the urbanity of the scholar. His subject-matter is either lately or presently under treatment. Scarcely has he commenced his discourse, when suddenly directing his attention to a neighbouring pupil, he calls upon him to supply a hiatus which he has purposely left, or probes into his private knowledge of the subject. If the pupil be unequal to the task, he passes on till some one more acute in memory or extensive in erudition solves the problem. If it should have been answered at the outset, a new question arises from the other, and passes on. One student is made to refute and inform another. The one sees his own deficiency; the other gains a laurel for his greater exertion. Finally, the lecturer concludes by giving all additional information on the subject, and, if necessary, adding his own opinion, which must be esteemed of no mean value. I may safely assert that nowhere is more valuable instruction given than within those walls, and I firmly believe that no body of men, who have obtained a license to practice, have a more perfect knowledge of this branch of science than the pupils of the Dublin Lying-in Hospital.—I am, Sir, &c.,

A LATE PUPIL.

November 17th, 1839.

* At present there is a daily dispensary, where advice and medicine are gratuitously given to a number of women and children.

## THE LANCET.

*London, Saturday, November* 30, 1839.

One of the consequences of the progress of civilisation, and the extension of cities, has been to render the burial of the dead a matter of considerable importance and difficulty. In a country that is covered with forests, herbage, grass, or dry sands, the savage, when he falls, may be devoured by birds of prey, or wild beasts may leave his bones to bleach in the air, or to be gathered by his kindred to the cave, the pit, the mound, the home of his fathers, and resolved into dust without any risk of injury to the living. The vapours of putrefaction in such a case mingle with the atmosphere, and are diffused until the diluted poison is absorbed, fixed, and again organised in the vegetable kingdom. But in a populous metropolis, such as London, where eighteen hundred thousand persons live in juxta-position, and fifty-two thousand die annually, the sciences which attend civilisation must be called into requisition to obviate an embarrassment and obstacle to its progress, or the dissolution of the dead will inevitably spread contamination and disease among the living. For death, like life, propagates itself.

The subject has hitherto been neglected in this country. Bodies are deposited in the churches, vaults, church-yards, and cemeteries, with becoming decency, but no one stops to inquire through what process they subsequently pass, and the public seem to take it for granted that the dead will lie harmlessly and tranquilly until "the resurrection of the just," provided the interference of the anatomist be averted. A work that has just appeared will tend to dispel this delusion,* and we shall avail ourselves of the facts which it contains, to present our readers with a short summary of the history of burial, and the effects of dead bodies on the living, and the practices pursued in London church-yards, with such remarks as may lead to a thorough examination and reform of the present system.

The question is not simply—In what way can the dead be disposed of with least inconvenience to the living? The feelings and prejudices of the people must at the same time be conciliated. It is a common reproach that medical men do not sympathise with the popular respect for the dead; that they are devoid of "feeling;" that, like the worm, "your great anatomist," they revel in reckless desecration, and have only an eye for the nerve and artery, the joint and sinew, but remain quite insensible to the tenderer emotions of our nature.

> "Physician art thou? One, all eyes,
> Philosopher! a fingering slave,
> One that would peep and botanize
> Upon his mother's grave?"

Nothing is more false or unjust than this imputation. The softer feelings of the heart are quite compatible with the exercise of the intellect; and if the medical philosopher investigates the causes and seats of diseases, discovers the means of disarming death and preventing sorrow, and seeks to render interment uninjurious to the living, and compatible with the health, happiness, and highest interests of mankind, he may smile at the sullen, ignorant sorrower, centered and coiled up in a supercilious, though poetical, sentimentality.

In the disposal of the body after death, the encouragement of virtue, the consolation of the mourning friends, and the exigencies of medical science and the public health, should be constantly kept in view; and in the history of burial it will be found that they have been more or less consulted, but, through ignorance, they have never all attracted due attention, the system of burial in no country having realised the results

* "Gatherings from Grave-yards, particularly those of London; with a concise history of the modes of interment among different nations, from the earliest periods, and a detail of dangerous and fatal results produced by the unwise and revolting custom of inhuming the dead in the midst of the living," by G. A. Walker, Surgeon.

which may now be regarded as fairly attainable. The Egyptians, according to Diodorus Siculus, held an inquest upon their dead, by the shores of Acheron, conveyed the good over the waters, and deposited them in pleasant burial grounds; while they precipitated the wicked into a loathsome pit, the prototype of the fabled Tartarus. This institution, if conducted with justice, would exercise a favourable influence upon the conduct of the citizens. Monuments, raised by the public gratitude, in antiquity and in the present day, signalise greatness; they stimulate the ardour of youth by example, excite industry, and teach the proud heart that nothing can endure so long as the memory of virtue and intellect. The honours of sepulture in St. Paul's, or Westminster Abbey, might exercise an excellent influence if they were distributed with due discrimination; and public monuments, in a National Pantheon, raised to such benefactors as Harvey, Sydenham, and Hunter—too much neglected in life—could never be visited without inspiring some thoughts of public usefulness. Great men are rare, and immortal names are told by centuries; but every father, after death, is to his own family what great men are to nations.

In public burial the satisfaction of the living must also be considered. This appears to depend a good deal upon arbitrary circumstances. The Egyptians embalmed, the Greeks burnt, Europeans inhume, their dead; the Greeks and Romans laid the ashes of the dead by the way-side; the Jews buried in "whited sepulchres;" the Turks have cemeteries, shaded by the dark cypress; Christians have hitherto, generally, employed the churches and church-yards as places of burial. The first impulse of the heart, when a friend has died, is, probably, to preserve the body entire; and this, in Egypt, and in other countries, may have given rise to embalming, which was also calculated to obviate the injurious effects of putrefaction upon the health in hot climates. The life—the stirring motion—the warmth—the intellect—the play of affection—are gone for ever; the colour has faded from the lips, the brightness from the eye; expression has sunk into "cold obstruction's apathy;" but the heart fondly asks if the form and material substance cannot be preserved? The mummy is a sad response to this question. It is too plain that any attempt to prevent the dissolution of the body is a fruitless mockery; and that the thoughts, the fruits of the actions, and the memory of the deceased, are all that can be preserved by survivors. Looking at the subject philosophically, it is evidently not desirable that the law of dissolution should be reversed. If the body were indissoluble, the millions who have lived and died—the petrified leaves of humanity—would cover the face of the earth, and render it a joyless desert of inanimation; while, by the present law, disorganization and organisation follow each other in an endless cycle, and there is a constant tendency in the organic elements to reassume vitality, in one or other of its multitudinous forms. Where are the ashes of our ancestors—where are the graves of the millions that have peopled this island, from time immemorial? Have the water, the gases, the dust of which they were compounded, remained inanimate, or have they not lived again, and seen endless metamorphoses? This is the law of nature, and it should be the object of an enlightened philosophy not to preserve the elements of the body in a state of revolting decay, but to facilitate their return to life as speedily as possible, and so far, as this is compatible with the other objects which we have said should never be lost sight of.

As the dissolution of the body is inevitable, there can be no just ground of objection to the *post mortem* inspection of the organs, nor to the dissection of its structure, for the purposes of anatomical instruction. Dissection, when decently conducted, does not interfere with sepulture; the undissected body goes through a fouler process in the grave or the vault, and the investigation of the healthy organs, with the changes which

they undergo in disease, is calculated to render invaluable service to mankind. It may therefore be expected that the prejudice (for prejudice it unquestionably is) against dissection, will gradually subside. Mr. WALKER gives some curious instances of the extent to which the feelings and prejudices of the public have been used by the Proprietors of Grave-yards. When the dread of the resurrectionist prevailed, grave-yards were erected,—surrounded with walls, and well watched,—in which the public were led to believe that the dead would never be disturbed. Thousands of bodies were interred within a very limited space, and still there was room: the grave-yard never filled, but by some system of "management" had always an empty grave at the service of the applicants. At the same time coffins, brass nails, and "furniture" were sold in the neighbouring stores; "many tons of human bones every year were sent from London to the north, where they were crushed in mills constructed for the purpose, and used as manure. (*Quar. Rev.*, No. 42, p. 380.) It is, in fact, quite evident that a wholesale system of resurrectionism was carried on, under the technical term "management," by the very parties who were most loud in their cries against the "body-snatchers," engaged in a more precarious but more useful trade. Scenes like the following, described by Mr. WALKER, are not uncommon in the London church-yards:—

"On Saturday the 27th April, 1839, at 5, P.M., I went, accompanied by a friend, to Nos. 30 and 31, Clement's-lane, and, upon looking through the windows of the back attics, we saw two graves open, close to the south-eastern extremity of this burying ground. Several bones were lying on the surface of the grave nearest to us—a large heap of coffin wood was placed in readiness for removal, and, at a small distance, a heap, covered with coarse sacking, was observed, which, when the covering was taken off, proved also to be long pieces of coffin-wood, evidently not in a decayed state. The nails were very conspicuous. Several basketfuls of the wood were taken to a building at the south-west extremity of the ground. We were informed that this sight was by no means a novel one; it was commonly, almost daily, observed. The cloth covering of the wood appeared to be nearly as fresh as when interred. The grave-diggers were seen to take off the plates from the coffins broken up."

We shall return to this subject.

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*Elements of Chemistry; including the Application of the Science in the Arts.* By THOMAS GRAHAM, F.R.S., &c., Professor of Chemistry in University College. Part III. London: Ballière. 1839.

In a former notice of this work, we laid before our readers a brief outline of the doctrines of chemistry, which have been recently discovered. In the present number of Mr. Graham's work, which contains 2½ parts, of the size of that which was previously issued, the same philosophic spirit is evinced by our author, the same disposition to present us with a general connected view of chemistry, as a branch of the great system of nature, rather than to overload his pages with isolated facts. Part II. concludes with the consideration of *chemical affinity*, the subject being discussed under the heads of cohesion—solution—order of affinity,—circumstances which affect the order of decomposition—the influence of insolubility.

*Inductive Affinity or Electricity.*—In the present number this subject is continued under the term "Inductive Affinity, or Electricity." This arrangement we consider to be an important improvement in the management of a course of chemistry. The phenomena of electricity, as usually taught, consisting of the description of apparatus, the exhibition of dancing men, the explosion of powder mills, &c., belong to a study which is preliminary to that of chemistry, viz., natural philosophy. But the silent action of the pile is, unequivocally, a phenomenon that is strictly within the bounds of chemical discussion. Chemistry is, in truth, the action of matter upon matter, at *insensible* distances; and voltaic electricity is but a variety of the same, or insensible action inducing *distant* effects. Under this view, the idea of an electrical fluid is rendered unnecessary.

In carrying out his chemical view of electricity, the author found it necessary to make corresponding alterations in the nomenclature. Thus, instead of *positive* and

*negative* properties, he proposes the terms *zincous* and *chlorous*. The terms *positive* and *negative* are founded upon the theory of Franklin, and have no association with the actual phenomenon, so that they are very liable to be confounded and misunderstood by students, and even by those who have paid a greater attention to the subject. For the poles it is proposed to take in consonance with the preceding alterations the terms *zincoid*, quasi zinc, or the positive pole the positive electrode, the anode and the zincode; and the *chloroid* as the negative pole, or negative electrode, cathode or platinode. When a body, such as water, is decomposed by the voltaic current, it is said to be an *electrolyte*, according to the electrical language of Faraday, and the decomposition is denominated *electrolysis*. The corresponding language, according to the chemical terminology of our author, will be *zincolyte* and *zincolysis*. We are glad that Dr. Graham has introduced the improvement of taking into consideration electricity, merely so far as it bears directly on chemistry, because, under the old system, the medical student's time was occupied in studying a branch of science which, compared with the phenomena of animal and vegetable chemistry, was trifling in value to him.

*Non-metallic Elements.* — These are the first simple substances which are treated of. The number of simple bodies now known amounts to 55, and that of the non-metallic elements to 13, comprising oxygen, hydrogen, nitrogen, carbon, boron, silicon, sulphur, selenium, phosphorus, chlorine, bromine, iodine, and fluorine. The various methods of procuring *oxygen* are carefully detailed. This is highly proper, because the black oxide of manganese at present in the market is so impure, that the oxygen prepared by its ignition is greatly deteriorated in its properties. In describing the properties of this substance, as well as of the subsequent substances, we might suggest that greater perspicuity would be attained by numbering them. It is this practice which gives so much additional value to the treatises of Thomson and Henry.

The author has introduced several modifications of the atomic weights, on the authority of Dr. Clark, of Aberdeen. Dr. Clark has taken the trouble of examining the different experiments made to determine the atomic weights by Berzelius, and has pointed out the important fact, that in these experiments between the highest and the lowest results, on different trials upon the atomic weight of the same substance, a very considerable interval exists; in some cases, as in that of sulphur, to the amount of upwards of one per cent. This may be viewed as a strong presumption in favour of Thomson's theory, that all atomic weights are multiples of that of hydrogen, for the numbers so corrected lie close upon the verge of these two limits. The number which Berzelius has adopted for the atomic weight of sulphur is 201.17. Dr. Clarke, however, considers the correct number, from his calculations, to be 200, the exact number of Thomson. The number for azote, given by Berzelius, is 177., while Dr. Clark makes it 176.2, or a mean between the former and that of Thomson, or 175. It is obvious, from the consideration of these facts, and many others with which we are acquainted, through the kindness of the learned Aberdeen professor, that the atomic numbers supplied by Berzelius are by no means to be considered as perfect, and that the proper mode of making approximations to the truth will be by varying the methods of experiment. The atomic weight of carbon, given by Berzelius, is 76.44; according to Dr. Clark it is 75.6; and according to Thomson, 75. The number of Dr. Clark is, therefore, nearly the mean between the numbers hitherto in use in this country and on the Continent; and seeing that it is so, it is obvious that the range of error in experiment lies at least between 75. and 76.44. Why, then, should dogmatism exist on either side, more especially when we find that the numbers given by Berzelius are not correctly deduced from his own data?

But a still more curious circumstance, which has been brought to light by the labours of Dr. Clark, is, that all the specific gravities of the gases deduced from the experiments of Dulong and Berzelius are incorrect. It would appear that they entrusted their calculations to some inferior arithmetician, who has made a constant error, we believe, in the third decimal place. These subjects, however, will be fully treated in a report by Dr. Clark, in the forthcoming volume of the *Transactions of the British Association for the Advancement of Science*.

*Nitrogen or Azote.*—The French, with na-

tional prejudice, have continued to ascribe the discovery of this important gas to Lavoisier, in the face of the clearest evidence that its existence was detected originally by Dr. Rutherford, of Edinburgh, in the year 1772. Dr. Rutherford examined expired air, and found that, after the carbonic acid contained in it was absorbed by caustic lye, "qui tamen restat nullo modo salubrior, nam quamvis nullam ex aqua calcis præcipitationem faciat haud minus quam autea *et flammam et vitam* extinguit." Dr. Thomson long ago attributed this discovery to its proper author, and the only foreign writer who has followed his example, we believe to be Liebig, who says, in his edition of Geiger's *Handbuch der Pharmacie*, p. 213, "Dr. Rutherford, in Edinburg, bewies 1772, die eigenthümlichkeit des steikstoffs." The detection of its presence in air was not publicly announced until 1775 by Lavoisier, although he states that he discovered it in 1773. This was, however, a year subsequently to the publication of Dr. Rutherford. Professor Graham has done proper justice to our countryman. But we might suggest, that if the dates of discovery were annexed to the respective authors, his excellent *Elements* would be benefited, more especially as we are aware that they have been partially, and will probably be wholly, translated into the French and German languages.

*Carbonic Acid.*—Much discussion has lately arisen with regard to the physiological action of this gas. We think, however, that the advocates on both sides of the question have exhibited so much of the feelings of partisans, that little trust can be placed in their arguments. Some tell us that there is no harm, that there can be no detriment to human life, from placing a focus of carbonic acid, evolving enormous quantities of that deleterious gas by the second, in immediate contact with the animal frame; others, that carbonic acid may exist to the extent of eight or ten per cent. in the atmosphere, without producing any bad effects by respiration; others, that three per cent. may be breathed with impunity, and that they have actually detected four per cent. in the atmosphere of a theatre. We cannot accept the statements of these ill-advisers as a correct expression of the truth. Let the experiments of Dalton and Saussure be examined for a refutation of the fallacy. Broughton, whose trials are quoted by Mr. Graham, has shown that an atmosphere of one or two per cent. will produce death; and Mr. Coathupe, in the pages of THE LANCET, has proved that in a room where not more than one per cent. (there might have been less) of carbonic acid was present, insensibility was produced, and death would have speedily supervened in his own person. We consider that, at the inquests which were held in cases where death was produced by certain stoves of great notoriety, the individuals who granted certificates in favour of the stoves should have been treated as culprits by the law.

*Phosphoric Acid.*—The author has devoted several pages to the consideration of this acid, and with great propriety, because it is by his important researches on this substance, that a new light has been thrown upon the chemical functions of water, which has cast additional interest over the whole of the science. It was found by Dr. Clark, that phosphate of soda, after being heated, afforded a precipitate of a different colour with nitrate of silver, to that which it gave before ignition. Berzelius became acquainted with the fact, and did not simplify it much by calling it *isomerism*. Mr. Graham has cleared up the circumstances, although the answer to the question why this change of colour, or why this change of function must still be—they are ultimate facts.

1. If a solution of pure phosphate of soda be precipitated by acetate of lead, the precipitate well washed, and then suspended in cold water and decomposed by sulphuretted hydrogen, the solution, after filtration, contains the tertryoxate of phosphoric acid, or $3HO+PO_5$; that is, phosphoric acid united to three atoms of water as a base, it may be, therefore, termed *tribasic phosphoric acid*. When this acid is brought in contact with a solution of nitrate of silver, the three atoms of water separate from the phosphoric acid, three atoms of oxide of silver take their place, and a yellow precipitate ensues, having the formula $3AgO+PO_5$. If instead of the term *decomposition*, a complicated word which has no definite signification, we employ the term *substitution*, and say that the three atoms of oxide of silver have been substituted for the three atoms of water, or oxide of hydrogen, the change which takes place

is readily understood; and it shows us that water fulfils the office of a base equally with, though in a weaker degree than, oxide of silver. This was the capital discovery of Professor Graham, which has thrown such an important light on the science of chemistry.

2. *Bibasic Phosphate of Water, or Phosphoric Acid.*—When phosphate of soda is ignited, dissolved in water, and crystallised, a new salt is obtained, which Dr. Clark, the discoverer, termed *hydrophosphate of soda.* If a solution of this salt be precipitated by acetate of lead, and the insoluble salt of lead washed and decomposed by sulphuretted hydrogen, phosphoric acid remains in solution, containing in union with it two atoms of water, or $2HO+PO_5$. It afforded a white precipitate with nitrate of silver, consisting of $2AgO+PO_5$. This has been termed *pyrophosphate of silver*, and perhaps this synonym is useful for the sake of association.

3. *Monobasic Phosphate of Water, or Phosphoric Acid.*—When biphosphate of soda is ignited, and then treated as in the preceding case, monobasic phosphoric acid is obtained whose formula is $HO+PO_5$. The common glacial acid is monobasic acid. Professor Graham originally termed this acid *metaphosphoric acid*, but we can discover no advantage to be derived from retaining the name at present. The nomenclature derived from the function is infinitely superior, and could scarcely be improved.

In our notice of this excellent portion of the *Elements*, we have adverted to such subjects as most excited our attention on perusing it. We recommend it to the students. Its careful study will satisfy the subscribers that, if its appearance has been unpunctually deferred, the care with which it has been executed affords a reasonable apology for the delay.

## WESTMINSTER MEDICAL SOCIETY.

*Saturday, November* 23, 1839.

Dr. Chowne, President.

STATE OF THE KIDNEY IN DROPSY FOLLOWING SCARLATINA.—STATE OF THE MIND IN SUICIDES. — THE MEDICAL AND MORAL TREATMENT OF PERSONS DISPOSED TO SELF-DESTRUCTION.

Mr. Streeter placed on the table the kidney of a child aged 5 years, who had died from anasarca succeeding to scarlet fever, with serous effusion in the cavities of the chest and abdomen, and in the substance of the lungs. The kidney was highly congested in its tubular portion. Dr. Bright had informed him that he had just had an opportunity of examining the kidney of a patient who had died under similar circumstances; the kidney was mottled, from irregular distribution of blood through its structure, with probable deposit. The first-mentioned case exhibited nothing worthy of remark, except that no medical treatment had been employed until near its termination; the parents of the child being scarcely aware that it had scarlatina. No urine could be obtained for the purpose of being tested.

Dr. R. Willis had paid particular attention to the dangerous disease alluded to. He had originally treated it simply by purgatives and diuretics, but having examined the kidney in three fatal cases, and found unequivocal signs of inflammatory action present, he had resorted to blood-letting with the best effects; indeed, in his opinion, depletion was the only mode of checking the disease. In the cases which he had mentioned, the cortical structure, though paler than usual, was exceedingly vascular, and when cut into, a stream of blood poured out; other portions were also very vascular. He had in these cases found urea in the blood, as well as in the serum effused into the different cavities. There was urea in the serum effused in Mr. Streeter's case. He believed, in these cases, that patients died from the formation of a coagulum in the cavity of the heart. His reason for supposing this to be the cause of death was, because he had observed that, up to a certain time, the action of the heart and pulse was proper, but that suddenly the action of the two became confused, and no distinct sounds of the heart could be detected.

Dr. G. Bird believed that, in the majority of cases of scarlet fever, the occurrence of anasarca might be prevented by keeping the patient in a warm room, and occasionally using the warm bath. By these means the function of the skin was kept up, and the renal disease prevented. Out of upwards of seventy cases of scarlet fever which he had lately attended, only four cases of anasarca had occurred.

Mr. Snow had seen several cases of scarlet fever terminate fatally by the occurrence of anasarca. He had examined one of these cases, and he found the kidney enormously congested.

THE MENTAL STATE OF SUICIDES.

Dr. Stone opened the adjourned debate. Dr. Johnson had classed suicides into, 1st, those who were insane; 2ndly, those whose passion for a moment had overcome their reason; and, 3rdly, those who reasoned

well before they destroyed themselves. He, Dr. Stone, agreed with reference to the first class; as to the second, he thought nothing had been shown to prove that the persons forming it were not at the time of the commission of the suicide, under a state of temporary derangement, carried on by some powerful cause over which they had no influence. The third class was that respecting which the greatest difference of opinion was likely to exist. He thought, however, there was plenty of evidence to prove that persons did commit self-destruction after the most deliberate reasoning upon the subject. Some of the great men of antiquity had been mentioned as coming under this class; among these, Cato was the most prominent example. Now, had Cato not killed himself, but lingered out his existence, like Bonaparte, on a barren rock, he would not have acted the part of a Roman, or died in accordance with the genius of his age. Where was the sign of insanity in Cato? Look at the manner in which he spent the night preceding his death—in reading Plato on the immortality of the soul, and reasoning with himself on the dialogue which condemned suicide. He then lay down and slept for awhile, and early in the morning stabbed himself in the breast; but the blow did not kill him instantly, and, during the painful struggle which succeeded, he exhibited the same inflexible determination to die. Governments had sanctioned suicide; and it was stated in the early history of Marseilles, that when a person had determined on destroying himself, he was required to state his reasons for doing so to a magistrate, and, if these were found satisfactory, he was supplied with a potent poison at the public expense. The Hindoo widow, under the influence of superstition, thought she was following her duty by sacrificing herself. Writers of learning and genius had sanctioned suicide; Montaigne, Rousseau, Madame De Stael, and Lord Shaftesbury, were of this number. Surely, if the act of suicide implied insanity, so did the promulgation of arguments in favour of it. A great number of cases could be brought forward in which distinguished literary men had committed self-destruction in a state of perfect sanity. Eustace Budgell, the coadjutor of Addison and Steele, and the author of the papers signed X in the *Spectator*, destroyed himself. Having filled his pockets with stones, he jumped out of a boat, whilst passing under London Bridge, and was drowned. He had previously arranged his affairs, and in his bureau was found a slip of paper, on which was written "What Cato did, and Addison approved, cannot be wrong." Creech, the elegant translator of Lucretius, had frequently stated during his labours on this work, that he would commit suicide when he had finished it, and he kept his word. Voltaire had said of him, that if he had been translating Ovid, he would not have destroyed himself. The melancholy end of Chatterton, "the sleepless boy who perished in his pride," was familiar to all. He, Dr. Stone, should class the causes of suicide as followed:—1. An habitual belief in the justifiableness of the act, and the rest that death would afford. 2. Disgust of life. 3. The love of notoriety. 4. The dread of punishment; and, 5. The apprehension of public shame. M. Guerry had given a table showing the different sentiments which were uppermost in the minds of individuals, at the time they had destroyed themselves. They had stated, that they enjoyed their reason; had meditated long on suicide; assigned various causes for the act; expressed kind sentiments to friends; desired to receive the prayers of the church; expressed their belief in a future state; prayed that publicity might not be given to their suicide; that it might be concealed from their children; gave instructions for their funerals; left mementos for their friends; desired to be buried in particular places; expressed their fear of exposure in the Morgue; and their confidence in divine mercy. Human reason, he, Dr. Stone, thought, capable of such solemn reflections, and manifesting such deliberation, surely could not be charged with aberration; he was, therefore, compelled to believe that persons might commit suicide from causes independent of insanity.

Mr. Robins condemned the mode in which the discussion had been conducted, and thought the main question, as to why Christian men in 1839 committed suicide, had been lost sight of. He looked upon one of the chief causes to be hepatic disease, and considered dram-drinking to be genuine suicide.

Mr. Streeter would make but a few remarks on the causes of suicide, and on some points connected with it, in a forensic and medical point of view. In the human character there were motives which restrained and which impelled to the act, but he wished to record his protest, in the most decided terms, against the decision to which many of the speakers had inclined, that suicide was a common occurrence among the sane. True, the act of self-destruction was not, *per se*, a *conclusive* test of insanity; still the act of suicide, *per se*, without any assignable motive, as escape from the consequences of crime, disgrace and punishment, or any such heavy calamity, was a *strong presumptive proof* of insanity; and he thought the medical evidences should be governed by the well-known principle of British law, that where there was doubt the accused should reap the benefit. His own opinion was, that when death occurred, the being was no longer the subject of punishment; besides, such punishment extended to the surviving, and, calmly

viewed as a preventive means, was it likely that he who had forgotten the restraints of religion, the ties of family and friends, and the duties of his station, would have been withheld by any such motives? No. He thought the suicide too often long harboured the thought before he executed the deed, and such views would lead him still more to secrete his intention from his medical attendant. He would, therefore, much rather invite these poor afflicted creatures to confide the distressing idea that haunted them to their medical advisers, with the almost certain hope of receiving relief from medicine, and from judicious moral and soothing treatment. This was no visionary scheme. The influence of hepatic diseases upon the mind had been properly alluded to, and was familiar to all. Take, for instance, jaundice, where depression of spirits was an invariable symptom. The leading preventive principle, in a moral point of view, was to excite a feeling into action strong enough to overcome the tendency to suicide, which was mostly a morbid one. Witness the effects of this in instances adduced by the author—the man attacked by footpads on his way to drown himself. Again, in the French army, when suicide had become prevalent among the soldiery, the motive appealed to by the master-mind of Bonaparte, in his order of the day, was not the fear of punishment, but the strongest in the soldier's breast, the sense of honour. "A soldier should not desert his post; life was a post; the soldier should not leave it by suicide." In the instance of the Milesian young women, the same ruling feeling was appealed to; a stronger emotion than that of suicide was aroused, but in a manner which marked the barbarity of the age and country; the principle of cure, however, was the same. He would encourage hope and remove despair in every possible mode; change of scene, and occupation in new and varied pursuits, were powerful means of removing morbid feelings; and, above all, to awaken those sentiments that lead to duty, to his Creator, his family, friends, and society at large. He hoped much from treatment where the idea was disclosed. He had in his pocket the copy of a letter written by a lady to her medical attendant, to thank him for having, by treatment, rescued her from the daily contemplation of the act. It was the production of an elegant mind, and she feelingly remarked, that her mother knew she was indebted to him for restoring her to health, but not that he had rescued her only child from an unhallowed grave.

Mr. WINSLOW rose at a late hour to reply. He contended that the object of his paper had been misunderstood, and that the main points of it had not been discussed. The chief question was not that which related to the sanity of the individual who committed suicide, but rather an inquiry into the physical diseases which accompanied certain mental conditions, and their treatment. He had not stated decidedly that insanity could be traced in all who committed suicide, but he said that the presumption was in favour of insanity, and this he still contended. He then entered into a long explanation of his ideas on the subject of mental aberration, in which he reminded the members that a man might be perfectly mad from a perversion of moral principle, while his *intellect* was perfectly sound. With reference to the proof of insanity in some suicides, the case of Cato had been frequently appealed to, but the speakers had taken the view of his character and conduct rather from poetry than history—from Addison instead of Plutarch. He contended that the description which the latter gave of the last hours of Cato, fully warranted the opinion that he was a lunatic. The latter part of Colton's life was that of a confirmed debauchee, and he showed other symptoms of insanity. Sir Samuel Romilly and Lord Castlereagh were both proved to have been mad. It had been stated that suicide bore a relation in extent to the spread of civilisation, but this was true to a certain extent only; for he contended that if the heart as well as the head was educated, suicide would be less frequent, and, under such a state of "civilisation," would possibly become extinct.

## MEDICAL SOCIETY OF LONDON.

*Monday, Nov.* 25, 1839.

THE Society was occupied this evening with a discussion on the pathology of rheumatism, which offered nothing particularly novel or striking.

## ST. BARTHOLOMEW'S HOSPITAL.

ANEURISM OF THE ARTERIA INNOMINATA (?)

There is now a young woman with a pulsating tumour in the neck, under the care of Mr. Stanley. She is of good constitution, and has come up from Wiltshire, where she has been employed as dairy-maid. States that she always enjoyed good health till about two years ago, when, after suddenly lifting a heavy washing tub, she felt as she says, "something go inside her." This was followed by some difficulty of breathing, and palpitation of the heart, and in about half an hour she discharged nearly half a pint of blood by the mouth. Since that time she has never been well.

There is now a pulsating immovable tumour behind the sterno-mastoid muscle,

sternal extremity of the clavicle, and sterno-clavicular articulation. It reaches to about an inch and a half above the clavicle. Its surface is uniform, its pulsation synchronous, with the systole of the ventricle, and perfectly *diastolic*, we mean equal, in every part of the tumour. This forms the chief ground by which the nature of the tumour is known, as solid tumours growing over and producing pressure on arteries, receiving and communicating their pulsations, do so *not* uniformly, but with greater intensity at certain points; the pulsation being generally greatest in the centre, and gradually diminishing as we approach the sides of the tumour. If the pulsation be diastolic, it must be an aneurism, or a tumour containing fluid lying over an artery. Hence the remarkable resemblance chronic suppuration of a cervical gland often has to aneurism.

In this case there is a distinct bruit de soufflet; but this is of little diagnostic value, similar sounds being produced whenever the calibre of an artery is in any way altered, so as to produce unequal tension of its parietes, and irregular currents of the blood within it. Of course, the pressure of any tumour would do this. We saw a case lately, where a diseased ovary communicated the pulsation of the external iliac artery, and was supposed to be aneurism of that vessel. On pressing it, however, towards the median line, so as to place it beyond the influence of the artery, it lost its pulsation.

This woman complains of irregular appetite, frequent fluttering and palpitation of the heart, and a peculiar sense of coldness in the centre of the chest. When she came in, the pulse in the right arm was stronger than that in the left. On Monday no pulsation could be felt in either wrist; but the carotid and anterior tibials were natural. Though the respiratory murmur is much stronger in the left than in the right lung, indicating pressure on the right bronchus, there is no other sign of disease of the arch of the aorta, no dysphagia, bronchial respiration, bruit de soufflet, or second centre of pulsation in the chest. General arterial disease could scarcely be expected in a young country-woman; but it is difficult to conceive how an aneurism of the arteria innominata could so affect the circulation of the upper extremities, as to stop both radial arteries, and leave those of the head and lower extremities unaffected.

She was sent to London with the idea that the case was a good one for tying the carotid and subclavian arteries on the distal side of the aneurism; but Mr. Stanley thinks there is so much obscurity about the symptoms, that such an operation is out of the question. She will, probably, remain for some time in the hospital. The case is well worthy of observation. We shall watch it, and report progress.

## ST. THOMAS'S HOSPITAL.

### ACUTE PHTHISIS—DEATH—POST MORTEM EXAMINATION.

George Butcher, a young man of clear florid complexion, light hair, blue eyes, delicate skin, and slight make, aged about 22, was admitted on the 29th of October, under the care of Dr. Robert Williams.

On admission, he was suffering from the most urgent dyspnœa, with palpitation of the heart, and great anxiety. He was seen by the apothecary, who ordered sixteen grains of the house mixture of ipecacuanha and tartar emetic. This soon produced vomiting, and the expulsion of a large quantity of mucus from the air tubes, and was followed by great relief. He took also two colocynth and calomel pills, and five grains of Dover's powder, at night.

Oct. 31. Dr. Williams saw him. No accurate or connected history of the progress of his symptoms could be obtained, further than that till six months ago he had enjoyed perfectly good health, at which time he jumped in a sack for a wager at a country fair, felt something give way inside him, and had since been liable to cough, and occasional shortness of breath. While the pupils were in the ward, he had an attack resembling one of spasmodic asthma, which soon went off. He states these kind of attacks come on several times daily, and he finds the most easy posture is that of leaning forward on his raised knees. Pulse 120, very irregular in diameter, and so very soft, as to be stopped by the slightest pressure of the finger. Tongue very thickly coated with a white fur; bowels opened by the pills; nothing unusual in the alvine evacuations; skin hot, not very dry; hectic flush on the cheeks; respiration 45 in the minute; cannot take a full inspiration without bringing on a sense of suffocation; voice catching and unequal from the rapid respiration; frequent cough, and free expectoration of mucus, differing but little from the mucus of ordinary catarrh. On percussion, the right side appeared a shade or two duller than the left; both were less resonant than might have been expected in so thin a person. There was puerile respiration in the upper part of both lungs; diminution of the vesicular murmur in the lower part of the left, anteriorly and posteriorly. In the right lung there was a crepitating rhonchus from the third rib downwards, heard more distinctly in the anterior than in the posterior part of the chest. It was not so fine, or so dry, as we often hear in cases of acute sthenic pneumonia, but resembled the passage of air though mucus, or fluid, contained in minute tubes. The urine was scanty and high coloured, depositing a copious fawn-coloured amorphous precipitate. He is very thirsty, and in a state of much mental anxiety.

Dr. Williams ordered him to be bled to six ounces, observing that, though this might possibly tend to hasten his end, yet it was his only chance.

*Calomel*, 1 grain;
*Tartar emetic*, ¼ grain. A pill twice a-day.

Nov. 1. Experienced considerable relief from the bleeding; has had no return of the spasmodic fits of dyspnœa; pulse 135, still very soft; respiration 36 in the minute. The moist crepitating rhonchus is now more audible in the posterior parts of the right lung than yesterday. The puerile respiration in the upper parts of both lungs more intense. The blood which was taken showed a slight sizy film on its surface; but it had not separated into serum and crassamentum, appearing a dark tremulous mass when the film was broken. Let him continue the pills, with the addition of a grain of opium to each.

2. Says he has had no sleep for three nights; has a very anxious expression of countenance; pulse 140; respiration 40 in the minute; cough and expectoration as before; greater dullness on percussion in the right infra-mammary region than two days since; crepitus less audible there, more so in the posterior regions; we heard it to-day for the first time in the left lung, in the inferior and lateral portions.

3. Much the same; sleep very much disturbed; pulse 140; respiration 36 in the minute; crepitus more distinct, and somewhat extended in the left lung; urine still scanty, high coloured, and loaded with precipitates. Continue the pills. The paroxysmal increase of the dyspnœa again frequent.

*Comp. spirit of sulphur: æther*, 3 ½;
*Aromatic spirit of ammon.*, 3 ½;
*Camphor mixture*, ℥i. A draught to be taken when required.

4. Receives temporary relief from the draught; much more gasping respiration than yesterday, 44 in the minute; pulse 150, as nearly as can be counted, very fluttering, soft, and irregular; cough easier; expectoration free, and in no way morbid but in quantity; the tongue has remained from the first very thickly coated with white fur, which is now cracked in the centre, showing a very red state of the mucous membrane; much thirst; considerable mental anxiety; dullness on percussion on the right side continues to increase, and the crepitation to diminish; crepitation increasing on left side. He was ordered four ounces of red wine in sago, daily.

During the next two days, 5th and 6th, he continued to get worse, the dyspnœa becoming more urgent, respiration more laborious, and the pulse so rapid and irregular, that it was impossible to count it. On the 6th, the dullness and crepitation were much the same as on the 4th, the vesicular murmur in the upper lobes of both lungs being intensely puerile; the dyspnœa became more oppressive, and he died at four o'clock in the morning of the 7th.

An examination of the body was made thirty-three hours after death. The following appearances were observed:—The lungs and heart occupied the same relative positions as in health; the pericardium was healthy, and contained about an ounce of fluid; nothing irregular in the size of the heart, or in the relative size of its different parts; pleura quite healthy, except a slight and not very old adhesion in the upper part of the left side; no fluid in its cavity; lungs large and dark coloured; upper lobes of both lungs apparently healthy; throughout the whole of the middle and lower lobes of the right lung an opaque yellow matter, something between cheese and wetted sawdust, was equally diffused; on squeezing the cut surface, or scraping it with the back of a scalpel, a thick bloody serum copiously exuded; this matter was also diffused, though not so thickly, or to so great an extent in the lower lobe of the right lung, which was also greatly congested; the posterior parts of both lungs were engorged with bloody serum, doubtless the result of gravitation, the "hypostatic hyperæmia" of Andral; the bronchial mucous membrane was unusually opaque, covered with slimy mucus, on removing which the membrane appeared highly vascular.

The liver was also very much congested; its serous envelop rather more opaque than natural, and dotted with a number of greyish white spots, perhaps incipient tubercles. There was no trace of tubercular matter in the liver, or in the intestines or mesentery, which were carefully examined. The head was not opened.

On examining a very thin slice from the anterior part of the right lung, by the aid of a powerful microscope, the opaque matter appeared to consist of an aggregation of small globular masses; some of them of a cloudy, semi-transparent, cobweb-coloured substance, others quite opaque and fawn-coloured, while some appeared like a minute particle of lymph encased in a gelatinous sort of capsule. The deposition had taken place, not into the air-cells themselves, but into the cellular vascular tissue between them.

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## REFLEX ACTION WITHOUT SENSATION.

*To the Editor of* THE LANCET.

SIR:—Dr. Carpenter is in error when he says, in your last number, "No case, proving unequivocally, what experiments on animals can only render probable, that reflex action may take place without sensa-

tion, had *then* been published by Dr. M. Hall or any one else."

To prove my assertion, I need only refer your readers to your own pages, Vol. II. for 1837-8, pp. 37-40. Dr. Macartney's and Sir B. Brodie's remarks were read to the Medico-Chirurgical Society in May, 1836. Mr. Barlow's and Dr. Budd's cases were read, in a paper by Dr. M. Hall, to the Royal Society, in February and March, 1837, *before* Dr. Carpenter's at the Royal Medical Society of Edinburgh. The merit of having adduced the first cases in illustration of Dr. Marshall Hall's views, indubitably belongs, therefore, to Mr. Barlow and Dr. Budd. Besides the cases of Mr. Barlow and Dr. Budd were observed by themselves, and as cases of reflex action without sensation; neither of which is true of Dr. Carpenter's.

Mr. Barlow has the additional merit of having perceived and avowed the importance and novelty of these views, *before* Müller had borne testimony to the same in his *Physiologie*, and so opened the eyes of *some* of our British physiologists. I am, Sir, your obedient servant,

Nov. 18, 1839. VERITAS.

## BOOKS RECEIVED.

A Treatise on Obstetric Auscultaton, by Dr. H. F. Naegele; translated by Charles West, M.D. London, 1839, Renshaw, 8vo, pp. 120.

[A complete essay on the subject.]

A Grammatical Introduction to the London Pharmacopœia, and a Key to Physicians' Prescriptions, &c.; by S. F. Leach. Third Edition. London, 1839: Hughes, 8vo, pp. 260.

[This little work will be found extremely useful to students, who have received but an imperfect classical education.]

Elements of Natural Philosophy, &c., by Golding Bird, M.D. Churchill, London, 1839. 8vo. pp. 407.

A Treatise on the Diseases of Infants, &c., by C. M. Billard; translated from the French, with an appendix, by James Stewart, M.D. Churchill, London, 1839. 8vo., pp. 620.

Gatherings from Grave-Yards, particularly those of London, &c., by G. A. Walker, surgeon. Longman and Co. London, 1839. 8vo., pp. 258.

THE Anatomist's Manual; or a Treatise on the Manner of Preparing all the Parts of Anatomy, by J. P. Maygrier, M.D.P. Translated from the last French edition. London, Henderson, 1839, 8vo.

## CORRESPONDENTS.

*A Student.*—Probably, the regulations of 1831; but he should ask at head-quarters.

Will any correspondent be kind enough to inform us of the name and profession of the gentleman who acted as Coroner in the case where a heavy deodand was given, and a verdict of "manslaughter" returned, in an inquest that was held in consequence of an accident which occurred on the Stockton and Darlington Railway? We make this inquiry, because we find that a motion was made, on Monday last, in the Court of Queen's Bench, for quashing an inquisition drawn up on the above occasion, in which the name and occupation of the Coroner were kept back by the reporter.

Communications received from *Mr. Barlow*, *Dr. J. R. Lynch*, *A Member of the College*, *Mr. Kempe*, and *Dr. M'Cabe*.

The *Nervine Balsam* is thus composed:—Fixed oil of nutmeg and beef marrow, of each 32 parts; essential oil of rosemary and Peruvian balsam, of each 2 parts; essential oil of cloves and camphor, of each 1 part; alcohol, 4 parts.

*A Barrister* has quite mistaken the object and tendency of the publication to which he adverts, and has, moreover, altogether overlooked the fact, that the publicity, having been refused by the senate, cannot take place. Besides, the question is *not* one of relative, but of *positive* proficiency. The publication would neither lead nor tend to the institution of the surmised comparisons. Under these circumstances, it will be unnecessary to publish the letter of our correspondent.

*M.R.C.S.*—It would be useless to publish the list of persons who are "practising" in Newcastle-on-Tyne, without possessing any documents in evidence of their fitness, other than their indentures of apprenticeship. As the law now stands, surgery may be legally practised without leave from any quarter; and, if the parties unite the occupation of "druggist" with that of "surgeon," after the manner of Mr. GREENHOUGH, it is, at present, doubtful if the law can interfere with their medical proceedings, excepting in cases of *mala praxis*. And certainly, if no breach of the law be committed by Messrs. C., D., S., F., and G., it is not for us to interfere, since it *may* happen that the parties are fully qualified, mentally and scientifically, to practise medicine without possessing either license or diploma. Let the law be altered. It is *that* which is in fault.

Received *A. B.*, *Mr. Simpson*, *Mr. Gray*, *A Practising Apothecary*, *An Old Pupil*, &c. *Verus* and *Dr. Jenner* next week.

The statements of *A Constant Reader*, respecting *Dr. John Fisher Murray*, relate wholly to private matters, which cannot with propriety be noticed in this journal.